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PLEISTOCENE GLACIAL ECOLOGY AND THE EVOLUTION OF "CLASSIC NEANDERTAL" MAN[1]

F. CLARK HOWELL

ISOLATION is one of the most important factors leading to evolutionary change. In nature, a variety of isolating mechanisms are at work. One generally distinguishes between geographical and physiological isolation although a number of other subdivisions may be made.[2] In one way or another, isolation results in failure of forms of one group to interbreed with those of another, and thereby gives rise to discontinuous variation. So long as there is interbreeding between individuals of a population, or between population units (demes), hereditary differences between individuals or populations will be absorbed and the breeding isolate will be of heterogeneous composition. Thus, "the only way to preserve the differences between organisms is to prevent their interbreeding, to introduce isolation."[3]

One of the most important barriers to gene flow between potentially interbreeding groups during the Pleistocene was that of climate. Simpson[4] has noted the importance of climatic factors with regard to intercontinental expansion of

1 Vallois (1952) has recently raised the problem of orthography with reference to the term "Neanderthal." The name was originally applied to the human remains found by Fuhlrott in 1856 in the valley of the Dussel, a tributary of the Rhine. This valley was linked with the poet Neander and thus the term "Neanderthal" (valley of Neander). Orthographic changes in German early in this century omitted the letter "h" following "t" in a number of words (thus, "th" changed to "t"; "Thal" to "Tal"). Writers in English and French have persisted in using the older orthography, but as Vallois notes, a standard usage should be adopted. Henceforth the term "Neandertal" will be used by the author.

2 Mayr, 1942; Dobzhansky, 1951.

3 Dobzhansky, 1941.

4 Simpson, 1947.

mammals over a Bering Strait land-bridge during the Tertiary and Quaternary, since "the migration route clearly led through a cold area and filtered out all animals that could not survive in a more or less rigorous climate." On lower taxonomic levels, Mayr and Rand have presented some of the avifauna evidence, and Deevey has recently summarized much of the data bearing on plant and invertebrate distribution as affected by Pleistocene glaciation.[5]

Heretofore, there has been no attempt to examine human evolutionary data for possible evidence as to the influence of climatic factors on hominid distribution and development in the Pleistocene. The reasons for this are perhaps twofold: too few fossils and too much emphasis on morphological evidence with little attention to geography and climatology. Although a number of fossils have been assigned a phylogenetically collateral position, the basis for so doing has been almost exclusively the result of comparative morphological evidence alone. Since most workers in human paleontology are primarily anatomists, there has been little effort to view human evolution with regard to paleogeography and paleoclimatology. The necessity for such studies is obvious and the lack of them is greatly lamented.

In an attempt to clarify the relationships of one important and fairly numerous series of fossils, so-called "Neandertal" man, I have suggested elsewhere[6] that one group, designated "classic Neandertals," was most probably an isolated descendant of an earlier and more widespread "Neandertal" type. The evidence previously discussed was primarily anatomical, and it is intended here to consider in greater detail the geochronological and paleoclimatological data bearing on the problem.[7]

"CLASSIC NEANDERTAL" DISTRIBUTION, DATING, AND MORPHOLOGY

"Classic Neandertal" skeletal remains are restricted in distribution to western and southern Europe. The best preserved and most numerous specimens are from the departments of Charente (La Quina), Dordogne (Le Moustier, La Ferrassie), and Corréze (La Chapelle-aux-Saints) in southern France, an area well-known for its many discoveries of Cromagnon man. Other remains have been found as far north as Belgium (Engis, Spy) and near Dusseldorf, Germany (Neandertal). In southern Europe, remains are known from central Italy (Monte Circeo) and from the rock of Gibraltar (Forbes Quarry and Devil's

5 Mayr, 1942; Rand, 1947; Deevey, 1949.

6 Howell, 1951.

7 This work has been assisted by a grant from the Wenner-Gren Foundation for Anthropological Research.

Tower). Some of the specimens (Le Moustier, Neandertal, and the Forbes Quarry skull) lack stratigraphic information and cannot be dated but are assigned to the "classic Neandertal" group solely on the basis of morphology. For the other remains, there is good stratigraphic and faunal data available so that accurate dating is possible.[8]

Hopwood surveyed the associated fauna of most of these fossils and Zeuner later reviewed both the faunal and stratigraphic evidence for dating these remains.[9] What follows is both a summary of their work plus additional data drawn from the original literature.

On the basis of faunal evidence alone, it is possible to assign a number of the remains to the Upper Pleistocene (post-Third glacial) since the faunal content at this time is a distinctive one indicative of an arctic biotope. As Zeuner points out:

> The chief mark of the upper Pleistocene is the cold faunas of the three phases of the Last Glaciation. By this time, immigration and adaptive evolution had supplied a large number of species well fitted for the periglacial biotopes. Many, present in small numbers in earlier cold phases, now become abundant; arctic fox, varying hare, lemming and susliks *(Citellus)*, caballine horses, woolly rhinoceros, reindeer, musk-ox, mammoth *(E. primigenius* s. str.).[10]

This is Osborn's Third fauna containing arctic, tundra animals.[11] It should be noted, however, that any one animal is not distinctive. For example, the Second fauna of the Pleistocene (Pohlig's *Elephas antiquus* stage), already contained the newly-arrived woolly mammoth (*E. primigenius*) which appears during the second maximum of the Third glacial[12] and apparently evolved from the earlier *E. trogontherii* characteristic of the Middle Pleistocene.[13] Similarly, the woolly rhinoceros (*R. tichorhinus* = *antiquitatus*), "a steppe and tundra form which immigrated into Europe from the east,"[14] is also found in the glacial deposits of the Middle Pleistocene, but in association with *E. primigenius* is especially

8 The fossils considered here include only those cases for which skulls and mandibles (and postcranial remains in some instances) are available. The La Naulette (Belgium) mandible and perhaps the child's skull from Pech de l'Aze (Dordogne) are also of similar age. Many other specimens of a fragmentary nature cannot be adequately dated, i.e., Arcy-cur-Cure (Yonne), Gourdan (Haute-Garonne), Isturitz (Basses-Pyrenees), Malarnaud (Ariege), Montmaurin (Haute-Garonne), Banolas (Spain).

9 Hopwood, 1935; Zeuner, 1940.

10 Zeuner, 1945.

11 Osborn, 1910.

12 Cf. Zeuner, 1945.

13 Soergel, 1912.

14 Zeuner, 1945.

characteristic of the Upper Pleistocene. It is thus necessary to consider the associated fauna as a whole and not rely on any one species as a key, and also to examine the relative abundance of each species within the total faunal assemblage.

In the most northern deposits at Spy in Belgium,[15] the most abundant associated mammals are woolly rhino, caballine horse, reindeer, woolly mammoth, and cave hyena, with cattle less frequent. Farther south, in southern France, at La Chapelle and La Quina, reindeer remains seem to be most numerous with abundant remains of caballine horse, cattle and/or bison; there is scarce representation of either woolly rhino or mammoth in the presence of a few molar teeth.[16] At La Ferrassie, the situation is similar but with primarily cattle or bison, some horse and deer, little reindeer, some boar, wolf, and fox.[17] This difference in faunal content (see Table 1 for the main faunal elements) is important because of the differing climatic conditions dependent upon proximity to the Scandinavian ice-sheet, a region of more continental climate and lower vegetation where large herbivores would be most numerous (see below).

On the shores of the Mediterranean, associated fauna is of less assistance because of persistence of temperate forms in the south during the time of the northern ice-sheets. At Monte Circeo, however, Blanc[18] has convincingly demonstrated the age of the "classic Neandertal" skull found in the Grotta Guattari near Rome. From the presence of the gastropod *Strombus bubonius* in the lowest beach deposit, Blanc has shown that the deposit is of post-Tyrrhenian age (= later than Second interglacial) and that such a fauna indicates the late Monastirian (= Tyrrhenian II) shore line formed during the transgression sea-level of the Third interglacial. The grotto could thus have been occupied only during the post-Monasterian regression with sea-level lowering during the onset of the Last glaciation. The fauna associated with the human skull (and a mandible of another individual) is essentially of woodland type, including leopard, bear, caballine horse, and ibex. Blanc believes that this must coincide with the first stadial of the last glaciation because the "intense cold of Würm II should already have begun at the time when the grotto had been closed by the rock-fall" which sealed the cave until its recent opening by workmen. Zeuner points out that the latest date possible for the human skull was "the beginning

15 Fraipont and Lohest, 1887. I have been unable to find any data on the relative abundance of each species at Engis (Fraipont, 1936).

16 Bouyssonie and Bardon, 1908; Boule, 1911-13; Martin, 1923.

17 Capitan and Peyrony, 1912; Perony, 1934.

18 Blanc, 1940, 1942.

TABLE 1

Mammalian associations with "classic Neandertal" fossil men
(modified after Hopwood, 1935)

Fauna	*Englis #2*	*Spy*	*Neandertal*	*La Chapelle*	*La Ferrassie*	*La Quina*	*Le Moustier*	*Monte Circeo*	*Gibraltar 1 (Forbes Quarry)*	*Gibraltar 2 (Devil's Tower)*
Elephas primigenius (woolly mammoth)	+	+				+		sp.?		
Rhinoceros tichorinus (woolly rhino)	+	+		+	+			sp.?		
Rangifer tarandus (barren-ground reindeer)	+	+		+	+	+		+		
Cervus elaphus (red deer)	+									
Capra ibex (steinbok)	+			+						
Bos primigenius (urus)	+	+			+	+		+		
Bison priscus (bison)	+			+	+	+				
Sus scrofa (wild boar)	+									
Equus caballus (caballine horse)	+	+		+	+	+		+		
Ursus spelaeus (cave bear)	+	+				+		sp.?		
Canis lupus (wolf)	+			+	+	+				
Hyaena spelaea (cave hyaena)	+	+		+	+	+		sp.?		
Arctomys marmota (alpine marmot)				+		+				
			No associations known				No associations known		No associations known	This fauna includes *Canis lupus*, *Hyaena crocuta*, *Sus scrofa*, *Cervus elaphus*, *Bos cf. primigenius*, *Equus caballus*; great auk, alpine and red-billed chough.

of the second phase of the Last Glaciation when thermoclastic weathering could for the last time have produced the quantity of rock-waste which sealed the cave."[19] It is interesting to note that a "middle layer" of the cave deposit (below the surface floor on which the skull rested) contained remains of *Hippopotamus (amphibius?)*, indicating a warmer climate prior to the human occupation period and apparently coinciding with the onset of the post-Monasterian regression.

The Gibraltar child's skull from Devil's Tower is difficult to date.[20] Neither the mammals nor the mollusk fauna alone are especially indicative, although these plus the avifauna provide some evidence. The beach deposits (8.5 m) at the base of the rock-shelter suggest correlation with the "lowest level of the *Strombus*-sea, the Late Monasterian of the Mediterranean shores":[21] the human occupation therefore postdates that time. The presence of the ibex suggests a woody vegetation but not necessarily a colder climate. The presence, however, of two birds (alpine chough and red-billed chough) found today in higher altitudes, plus the ibex, "lends support to the view that these beds were deposited during the incipient stage of the Last Glaciation, after the sea-level had begun to recede from the Late Monastirian level," and that the skull is therefore "contemporary with that from the Monte Circeo in Italy."[22]

Thus far, the faunal data presented indicates that the "classic Neandertals" are found in Upper Pleistocene deposits indicating cold climatic conditions north of the Mediterranean (France and Belgium) and in the Mediterranean area a temperate wooded environment following the Late Monastirian regression and the onset of the Fourth glaciation. Stratigraphic data offers additional confirmation of this dating and makes it possible to assign the fossils to the first stadial of this glacial.

At Spy, the human skeletal remains rested on a weathered brown, sometimes, black clay, containing limestone pebbles,[23] indicating temperate conditions.[24] The layer above and including the skeletons was a "yellow limey-clay," "presumably of loessic type,"[25] which would correlate with the Younger Loess I[26] of northern Europe, deposited during the last glaciation. This is apparently confirmed by the upper levels where there is a reddish, apparently weathered layer (interstadial), followed by a yellow clayey tufa representing the Younger Loess II.

19 Zeuner, 1950.
20 Cf. Bate, 1928; Zeuner, 1940, 1945.
21 Zeuner, 1945.
22 *Ibid.*
23 Fraipont and Lohest, 1887.
24 Zeuner, 1940.
25 *Ibid.*
26 Soergel, 1919.

The top of this layer of fallen stone and brown clay equates with the "intense frost-weathering" during a cold period (which Zeuner also regards as the second advance of the Last glacial). This site offers almost conclusive evidence that the human remains were contemporary with the first stage of the Fourth glacial and not of earlier or later date.

In southern France, loess deposits are not found,[27] and it is necessary to seek other evidence of climatic alterations. The La Ferrassie rock-shelter offers an excellent stratigraphic sequence, although it has not as yet been carefully analyzed geologically. The basal layer is composed of small lime elements of reddish color which may represent weathering. The fauna here is predominantly horse, with less numerous cattle and deer, indicative of a temperate climate.[28] Almost at the surface, and near the base of the succeeding layer, are found reindeer teeth, indicating that the climate had become colder with the onset of the glacial stage which is fully present in the following layer containing the human skeletal remains. This is perhaps substantiated by a layer of yellowish limey sands apparently from disintegration of the walls and vault of the shelter (frost-weathering) interposed between the basal layer and the human skeletal remains. The brown earth layer containing the skeletons is indicative of a coniferous forest region, as is suggested by the fauna. The transition here from temperate to cooler conditions appears to indicate the onset of the Fourth glaciation. Reindeer become more abundant in the following layer, and as Zeuner points out, "this distinction is characteristic of the cold phase Würm I and Würm II. . . ."[29]

Both at La Chapelle and La Quina there is stratigraphy but no data other than fauna which could make it possible to date the human remains accurately. The presence of an Upper Pleistocene cold fauna, plus the evidence from Spy and, more particularly, La Ferrassie, make it seem probable that here too it is a question of the first stage of the Fourth glacial.

The available evidence suggests that all "classic Neandertals," which can be dated, were *approximately* synchronous and lived in western and southern Europe during the first stadial of the Last glaciation. These are the first known remains of a man in Europe living in a cold area during glacial times, all earlier hominids from Europe now known being associated with warm interglacial fauna and flora. Following the "classic Neandertals," however, are abundant remains of anatomically modern man found with cold faunas in western Europe during the later stadia of the Fourth glaciation.

27 Grahmann, 1932.
28 Peyrony, 1934.
29 Zeuner, 1940.

It has been noted previously[30] that the "classic Neandertals" possessed a cranial morphology which can only be regarded as most peculiar and distinct. This was noticed in individual specimens by Schwalbe, Boule, and Martin, and surveyed in a number of specimens by Morant, to mention but a few.[31] Both Weidenreich and Hooton defined the group primarily on the basis of morphology, with slightly different conclusions,[32] but here the delineation of the group is based both on morphology and on chronology. Boule and Klaatsch also noted the distinctive features of the post-cranial skeleton.[33] The most striking anatomical features of the "classic Neandertals" may be briefly summarized as follows:

Long, low and wide cranial vaults; poorly flexed cranial bases; rather sharply angulated occipital bones with heavy occipital tori; horizontal orientation of the tympanic plate with heavy anterior and posterior portions; large facial skeletons; semicircular supraorbital tori with fused medial and lateral elements; shelving of maxillary bone into malar with no clear demarcation between these bones and lack of maxillary hollowing into a canine fossa; short massive vertebral column; cervical vertebrae with long spinous processes projecting perpendicularly to the vertebral body; reduced cervical and lumbar curvature; narrow, slightly arched sacrum set deeply between ilia; long and slender (but robust), considerably arched clavicles; scapulae with suleus ("Boule's fossa") on dorsal aspect of axillary border; robust, short humerus with massive head; pronounced curvature of the radius; marked development of olecranon process of ulna; pelvis with flattened ilia, great length relative to height; much bent femora with massive cylindrical shafts; strong, short tibiae with retroverted heads and supplementary malleolar facets; calcaneum with well-developed sustentaculum tali; forearm short in proportion to upper arm, and leg very short in relation to thigh.

The significance and origin of this interesting morphological pattern will be considered following a discussion of the climatic conditions in the "classic Neandertal" area and surrounding regions.

AN ISOLATING BARRIER: CLIMATE

Although the Fourth glacial age was apparently not as severe or as extensive as the previous glaciation, "in northern Europe, between latitudes 50 and 70, maritime air masses built up an ice-sheet that got its start in the Scandinavian mountains and gradually spread over an area of 1,650,000 square miles."[34] The maximum extent southward of the first phase, represented in northern Europe by

30 Cf. Howell, 1951.
31 Schwalbe, 1901; Boule, 1911-13; Martin, 1923; Morant, 1927.
32 Weidenreich, 1928, 1940, 1943; Hooton, 1946.
33 Boule, 1911-13; Klaatsch, 1901.
34 Flint, 1947.

the Warthe drift (or the Brandenburg drift?),[35] extends from the German North Sea coast towards the east through central Poland into western Russia. Although this was the ice-sheet margin, there was to the south, "a zone surrounding the ice-sheet, in which the cooling effect of the ice produced a *frost climate*"[36] with severe *periglacial* conditions. This area, marked by dune building, loess deposition, mechanical weathering, ground-ice structures, solifluction, and stream terracing,[37] has received extensive study only during the last twenty-five years with the result that past climatic conditions are now fairly well understood.

Kessler pointed out some years ago that climate in the periglacial region was predominantly cold with northerly ice winds, considerable temperature fluctuation (but annual mean temperature not above —2°C) and with relatively low precipitation.[38] He did not, however, attempt to delineate more completely climatic differences within the periglacial area. Zeuner delimited three main climatic zones: a northern tundra belt near the ice-sheet margin, a southern forest belt, and a central, broad loess-steppe between northern tundra and southern forest.[39] In the past few years, periglacial studies in Germany have made possible more detailed reconstructions of Pleistocene climatic zones during the Fourth glaciation.[40]

For the reconstruction of climate at this time, it is important to recognize three main factors leading to the changes: depression of the snowline, lowering of temperature, and probably, increase of precipitation. Directions of prevailing winds would appear to have been the same as at present, although there was a major shift toward the equator of the zones as known today.[41] Thus, the northern ice-sheet reached 55° N (present 77°), permafrost and tundra 45° N (present 69°), non-tropical forest 36° N (present 45°), Mediterranean vegetation 28° N (present 32°).[42] Compared to a postglacial vegetation map,[43] the differences are striking indeed.

Through a study of periglacial features (ground ice wedges, block fields, and asymmetric valleys), Poser has reconstructed the permafrost boundary indi-

35 Woldstedt, 1942, 1950. There is still disagreement as to the position of the Warthe drift, Woldstedt (cf. also Gams, 1938) assigning it to the late Saale (Third) glacial, other workers (like Zeuner, 1945) to the Last glaciation (cf. Flint, 1947).

36 Zeuner, 1945.

37 Smith, 1949.

38 Kessler, 1926.

39 Zeuner, 1937.

40 Poser, 1947a,b; Büdel, 1949.

41 Büdel, 1949.

42 *Ibid.*

43 Such as that found in Clark, 1952.

FIG. 1. Europe during the first advance of the Fourth glaciation. (Climatic zones after Poser, 1947b; enlargement of continental land mass to 90 meter isobath after Klute, 1951 and Pfannenstiel, 1951; Scandinavian ice-sheet after Antevs, 1929 and Woldstedt, 1950; Alpine and Pyrenean mountain glaciation after Antevs, 1929.)

cating the southern extent of perennially frozen soil (*tjaele*).[44] This line extended from Mount St. Michel eastward into the Seine-Marne area, curving southward to the western margin of the Alpine ice; east of the Alps, the boundary curved southward as far as the Serbian highlands and then continued eastward to the southern coast of the Black Sea. In western Europe, the polar forest boundary lay south of the permafrost line, extending eastward somewhat south of the Alps, then in the region of Agram turned northward through Moravia, the southern Carpathians, and north of the Black Sea. (Büdel extends this line much farther north in Russia along the eastern margin of the ice-sheet and coterminous with his 10.5°C July isotherm.) The crossing of permafrost boundary and polar forest boundary east of the Alps means an overlapping of forest growth on permanently frozen soil in eastern and southeastern Europe. (Poser points out that there are analogous conditions in Eurasia today.) With these boundaries demarcated, it is thus possible to work out climatic zones during the Last glaciation in Europe.

Poser distinguishes four major climatic zones (Fig. 1).[45] Most extensive is a permafrost-tundra region extending in a broad band from the British Isles (linked to the continent at this time because of lowered sea-level) over central Europe into Russia. This can be further subdivided into three subzones: a western glacial-maritime, a central transitional glacial-maritime, and an eastern glacial-continental. In this permafrost-tundra region, the July mean was below 10°C (about 3° to 6°) with a yearly mean below —2°C (permafrost climate ranging from —15° to —25°). Winters were cold and summers were cool, with maritime climate to the west and increasing continentality to the eastward. In central Europe, there was intense cold with heavy evaporation resulting in somewhat, but not extremely, dry climate. The central transitional zone had the most uncongenial climate, with heavy cloud formation in the brief summer months, high ground moisture, and snowfall; during the long winters, it was somewhat dry as a result of blowing and drifting snow over the light tundra vegetation. Poser regards loess deposition as taking place during the winter as a result of weathering and glaciofluvial action at the margins and outwashed deposits of the ice-sheet. Klute points out, however, that this would occur primarily during the summer when the westerly winds would be able to pick up such weathered material.[46]

Both Penck and Zeuner, among others, regard loess deposition as taking

44 Poser, 1947a, 1947b.
45 Poser, 1948.
46 Klute, 1949.

place under somewhat arid or semi-arid conditions.[47] However, as Flint points out, "it does not seem necessary to demand a semi-arid climate for the accumulation of the loess" since it does occur, though uncommonly, in fairly moist regions so long as there are abundant outwash sources from which the loess may be derived.[48] Thus, all that is required is that a region from which loess is picked up by winds "should have been largely free of vegetation instead of protected by it, as it is today."[49] Such would have been true of the ice margins with their extensive outwash and drift deposits and supraglacial accumulations of debris. It should be noted, however, that the "rodent fauna of the European loess suggests both arctic tundra and steppe conditions,"[50] so that the tundra climate may have been drier, but not necessarily so.

Zeuner has pointed out that in this subzone, "the middle European lowlands . . . were covered with a grass steppe with patches or stripes of woods in protected places along rivers and in the entries of the mountain valleys;[51] . . . the German loess must have formed a biotope that offered fairly propitious conditions of life to a fauna of animals adapted to the cold of the winter," this being "confirmed by the numerous fossils found in the loess."[52] Since the tundra regions were largely treeless with only shrub and ground vegetation, it would appear that an animal like the woolly mammoth and woolly rhino would adhere "to some region where he might obtain all the brush required, as he could on the extensive plains of both continents in summer, as well as among the small branchy trees at the edge of the forests in winter; the great length and complete curve of the tusks of these animals show that they were only fitted for traveling in such regions or in very open woods";[53] such animals would thus have extreme difficulty in making their way through the more dense coniferous forests farther to the south. In light of the faunal contents at some of the sites in France, this is an important point.

The tundra zone nearest the northern ice-sheet was an area of dune sand, sandy loess, and outwash deposits, with vegetation restricted to patches and bolsters of ground foliage.[54] Except for the period of exposed vegetation during the brief summer, this area was probably to a large extent unoccupied by

47 Penck, 1948; Zeuner, 1945.
48 Flint, 1947.
49 *Ibid.*
50 *Ibid.*; cf. Nehring, 1890.
51 Cf. Büdel, 1949.
52 Cf. Wernert, 1928.
53 Bell, 1898.
54 Zeuner, 1937.

mammals during most of the year because of frozen surface snow forming a hard crust which made feeding extremely difficult. Farther southward, however, the loess-tundra zone was an area well-stocked periodically with Pleistocene mammals and offered rich hunting grounds for early man. In spring, with snow melting and additional solar heat, there was fairly heavy shrub and grass vegetation, although still cold. At this time, the area would have been a hunter's paradise with gathering of numbers of mammoth, rhino, reindeer, and horse. In autumn, however, with precipitation and falling temperature resulting in heavy snow cover, this area, too, would have become somewhat unfavorable and mammals would have moved into sheltered mountain valleys or farther south into the more heavily vegetated areas.[55]

In the more eastern area of the permafrost-tundra zone, the summers were warmer than central Europe with somewhat less cold winters. Büdel points out that in this eastern subzone (from the Dnieper to the Urals, and from the Caspian Sea far northwards), there were trees of subarctic type (pine, birch, and willow) fairly widespread because of the milder summers but these were present as isolated clumps of trees rather than closed forests.[56]

In southeastern Europe, with the overlapping of forest on permafrost soil, there was a continental permafrost-forest climate with larch, birch, and pine forests east of the Alps. Poser has calculated temperature ranges for this region and finds a winter mean of —12.7°C (January mean —14°) and summer mean of 8.7°C (July mean 10°).[57]

In southwestern Europe there were two zones, the southernmost martime forest climate (without permafrost) extending over the southern part of France, and north of this region a narrow zone of maritime-tundra climate (without permafrost) separating the maritime-forest zone of the southwest from the northern glacial-maritime province. The maritime-forest province had a yearly mean temperature between 5°C and 7°C (July mean 10° to 13°, January mean 0° to 3°). Closed forests were restricted apparently to the southern part of France (pine and birch), the more northern maritime-tundra zone having a light subarctic forest and shrub vegetation.[58]

In the northern Mediterranean region, the present middle and north European subarctic vegetation prevailed, with pine, birch, larch, and willow, but lacking in warm plant species. South of the Pyrenees and Apennines, warm species of the

55 Cf. Soergel, 1940.
56 Büdel, 1949.
57 Poser, 1948.
58 Büdel, 1949.

present central European type were present.[59] Büdel also notes that it must not have been very dry here during the summer—witness the perennial vegetation—and that this indicates rain-bearing westerlies during the summer months; differing, thus, from the present Mediterranean climate where the summer is almost rainless and most of the precipitation falls in the cooler seasons. There was apparently fairly heavy rainfall in the area along the Mediterranean coast including North Africa and the region of the Sahara, with the modern Mediterranean flora extending into Syria and Palestine[60] and probably North Africa south of the Atlas mountains.[61]

Although an understanding of the directions of prevailing winds and the areas of high and low pressure are extremely important in reconstructing past climatic conditions, there is rather meager evidence available. Büdel states that there was present the same general system of winds during the Pleistocene as there is at present.[62] Although this was probably the case, there would have been a shifting of both climatic zones and winds towards the equator because of the extension of cold conditions away from the poles. This would bring "the 'polar front' between the polar east winds and the temperate west winds nearer to the equator, and by increasing the temperature contrast between low and high latitudes would increase the storminess" and would further result in "a small but intense anti-cyclonic belt in about 20° to 25° latitude, a narrow belt of powerful trade winds, and a deepened equatorial trough of low pressure."[63]

Klute has recently worked out the pressure zones and directions of prevailing winds for the Fourth glacial maximum (Figs. 2 and 3) as follows:

> In summer air currents seldom moved over the Mediterranean except for the current dVb. The low moved more strongly along dIV along the edge of the ice toward the northeast, bringing precipitation probably to the northern side of the ice-sheet. The winds were chiefly west winds, both on the front as well as on the reverse of the cyclone or polar front. Since the pressure differences through the high pressure region on the Fennoscandian ice-sheet was greater than that of central Europe today, the winds were also stronger. The dry winds of the reverse portion of the front were likewise westerly winds, carrying loess and forming dunes. Currents dIa-d were also active during the summer.
>
> In autumn, currents dIa-d were stronger and brought precipitation to the western

59 *Ibid.*
60 Picard, 1937, 1938.
61 Büdel, 1949.
62 *Ibid.*
63 Brooks, 1949.

portion of the ice-sheet. Current dId (as at present in winter) also passed northerly around the ice-sheet and invaded the continent, bringing precipitation to the northeast of the ice-sheet. The Mediterranean currents were active in autumn and influenced central Europe, from whence the westerly winds moved. Occasionally, however, the cyclone dIV moved into central Europe bringing snow. In winter, the cyclonic activity was reduced in central Europe, a high pressure area with still winds or with northwesterly to westerly winds prevailed, from the currents dVa-c. Also, at times, easterly winds came into the west from minima west of the British Isles. The ground was frozen deeply and covered with snow.

In spring, the high pressure descended over central Europe; current dIV gradually gaining ascendency, also dVa-c still active, plus dIa and now especially with its offshoots dIb-dId.

The pressure differences in terms of cyclonic activity was greater than at present and thus there were frequently stronger winds owing to the Fennoscandian and Alpine ice-sheets.[64]

The prevailing climatic conditions in non-glacial areas most certainly had a marked effect on the distribution of the Neandertal food-gatherers, and on the faunas and floras off which they lived. Severe weather conditions throughout most of the year in central Europe made this area uninviting and inhospitable for Neandertal man. Although there were undoubtedly movements of people from central and southern France northward during the summer months in quest of game, these were probably only periodic and of limited duration. The maritime-forest climate of southwestern Europe obviously offered more suitable living conditions than that of the colder northern periglacial regions. Both the climatic factor and the presence of numerous caves and rock-shelters with streams nearby made southern France the place in which to live. The Italian and Iberian peninsulas likewise offered sheltered and genial surroundings for human habitation.

While it is most improbable that any movements of people were made through the central European corridor, there is a distinct possibility that such may have occurred north of the Adriatic Sea through northeast Italy to and from southeastern Europe. This is especially so since with sea-level lowering (probably about 90-100 meters), additional land would have become continental.[65] The subarctic forest present at the foot of the Alps, and extending eastward, probably sheltered a considerable fauna making for plentiful hunting, and thus have given cause for occupation and movement. The extent of such movement remains entirely unknown because of the postglacial marine transgression.

64 Translated from the German of Klute, 1949; see also Klute, 1951.

65 Cf. Blanc, 1937; Pfannenstiel, 1951.

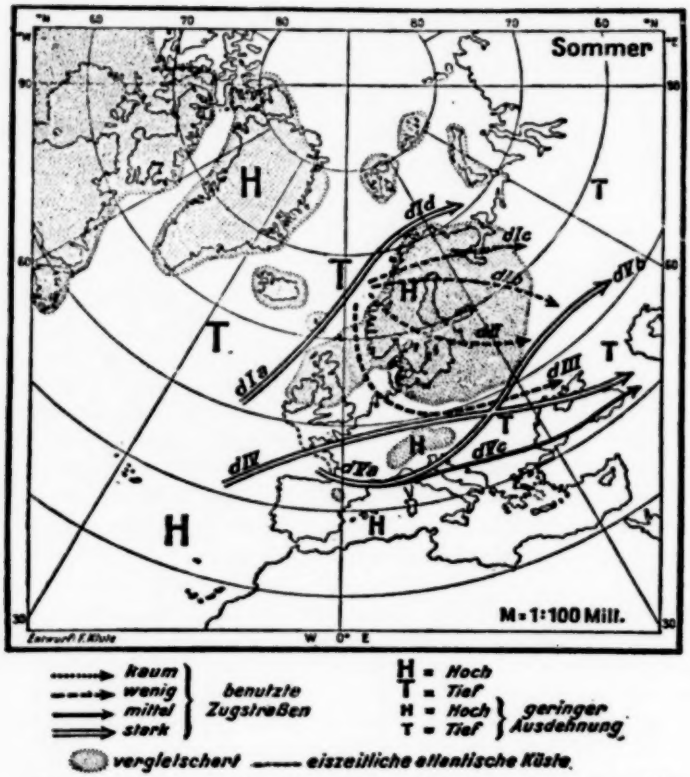

FIG. 2. Direction of prevailing winds during the Fourth glaciation—Summer (after Klute, 1951).

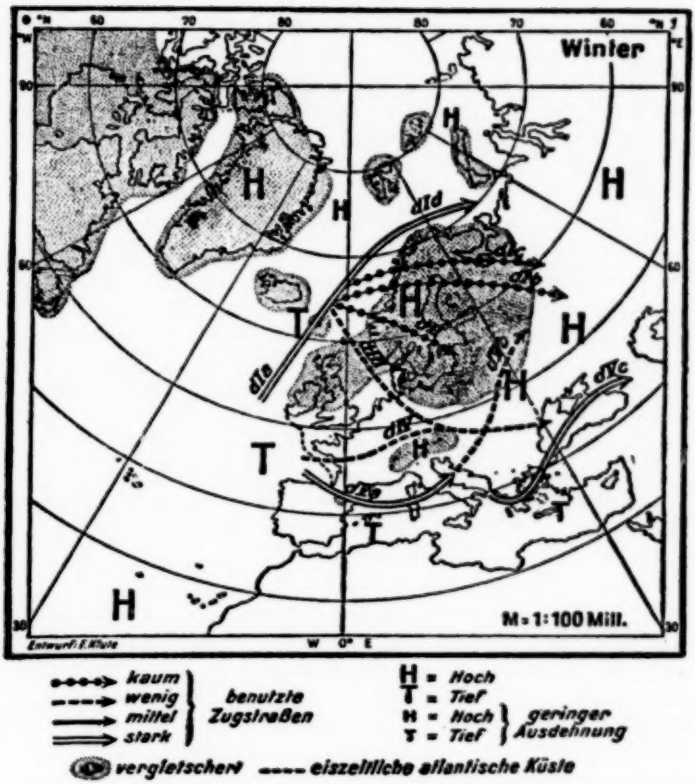

FIG. 3. Direction of prevailing winds during the Fourth glaciation—Winter (after Klute, 1951).

CULTURAL EVIDENCE FOR HUMAN DISTRIBUTION IN EUROPE IN THE FIRST STADIAL, FOURTH GLACIATION

It is necessary to examine the available archaeological evidence for further information of hominid distribution in central Europe during the first advance of the Fourth glaciation. In every instance in western Europe, the "classic Neandertals" have been found associated with so-called "Mousterian" flake implements. Yet there are a number of complications with regard to the concept of "Mousterian" as a distinct industry and there are major difficulties in terminology.

In France, there is one classification into "Mousterian" I, II, III, IV, beginning towards the end of Riss-Würm interglacial and persisting after the first advance of the Fourth glacial. There is also classification into lower, middle, and upper "Mousterian" rather like the foregoing. But to greatly complicate the picture, a number of other terms have been introduced to describe various assemblages of "Mousterian-like" artifacts. Some of these are: "Mousterian of Acheulean tradition," "Acheuleo-Mousterian," "Levalloiso-Mousterian," "Mikromousterian," "petit Moustérien," "Moustero-Tayacian," "typical Mousterian," "proto-Mousterian," and so on. Bordes has recently attempted to clarify this problem by analyzing statistically unselected tool assemblages in terms of the technological methods used in the production of flakes, and quantifying proportions of various flake and bifacial implements within a homogeneous assemblage.[66]

The "Mousterian" has been divided by Bordes into two main groups: "Mousterian" with bifaces and "Mousterian" with few, or without any, bifacial implements. Each group is further subdivided into "Levalloisian" or non-"Levalloisian" (prepared or unprepared striking platform) techniques, and additional categories are present in each subgroup. The characteristics of each group, subgroup and category can be expressed statistically in terms of the technology employed in manufacturing the implements, and the relative proportions present in the assemblage. This work has greatly clarified the "Mousterian" problem in terms of more exact delineation of assemblage content. It will be interesting to see the method applied to other assemblages from sites outside of France.

The problem of the "Micoquian" still remains and it should be noted that McBurney points out "that the whole conception of a Micoque stage as such rests on a distinctly flimsy and uncertain basis."[67] Here again a re-analysis of the older material is essential for a clarification of the relationships between

66 Bordes, 1950; Bordes and Bourgon, 1951.
67 McBurney, 1950.

unifacial and bifacial implements with emphasis on unselected assemblage content and geographical and chronological range.

McBurney has recently pointed out distinctions between the "Mousterian" of western Europe and that of the eastern and central portions of the continent.[68] He finds a localized occurrence of the "cordiform hand-axe" between the Loire River and the Pyrenees, and in Brittany and Belgium (also possibly in the west German plain). In central and eastern Europe, on the other hand, the "Mousterian" artifacts "virtually all show signs to a greater or less degree of that characteristic Middle Pleistocene device, faceted-platform flaking from miniature disc-shaped cores" with an absence of cordiform hand-axes. The geographical approach employed by McBurney, coupled with a careful analysis of assemblage content based on the technique developed by Bordes, promises to provide significant results on cultural distribution and human activities during this phase of the Pleistocene.

There was, of course, widespread occupation of maritime western Europe at this time, and the "Mousterian" is well-known in Italy, Spain, southwestern and central France, Belgium, and in parts of southern England. This more than covers the area from which "classic Neandertal" remains are known although it should be noted that a large region extending to the south and east of the British Isles linked the latter with continental Europe and made available land which is today under water due to marine transgression following glacial retreat.

The archaeological situation in Germany is still considerably complicated.[69] Andree summarized the German evidence some years ago but was very uncritical, and many of the sites which he attributed to the first stadial of the Fourth glaciation are inadequately dated or dated not at all, either faunally or stratigraphically.[70] The only well-dated German sites from this time follow[71] (Fig. 4):

In the northern Eifel mountains, just northeast of Luxembourg, "Micoquian" and "Mousterian" implements are known from the cave of Kartstein (1), dated as early Fourth glacial from a careful analysis of fauna, flora, and stratigraphy. To the south, in Würtemburg, two sites are known from the Lonetal in the Schwäbian Alps: both Vogelherd and Bocksteinschmiede (2) are well-stratified

68 *Ibid.*

69 Zeuner (1950) notes the lack of good evidence "in the formerly glaciated area of central Europe" where "the interval between Saale (Third glacial) and Weichsel (Fourth glacial-second stadial) is still to be regarded as a chronological gap from the archaeological point of view." The recent summary by Zotz (1951) provides for the first time an excellent critical appraisal of the central European evidence.

70 Andree, 1939.

71 Cf. Zotz, 1951.

caves with early Fourth glacial "Micoquian" and "Mousterian" (the latter slightly later in both caves) in strata containing limestone fragments. In the northwest Swiss Juras near Neuenberg, the cave of Cotencher (3) contains a "Mousterian of Acheulean tradition" superimposed on a Third interglacial yellow clay layer, the former being then of pre-maximum Würm I. These sites are all within the western maritime zone and constitute the most eastward expansion of the early Fourth glacial inhabitants of western Europe. Both Sirgenstein and Schulerloch, caves in the Altmühltal, Fränkischen Juras, considerably to the east, containing a "Kleinmousterian" and a "Mousterian of Acheulean tradition" respectively, are post-maximum of the first Würm advance or are already interstadial.

Benet-Tygel notes in eastern Europe, "a distinct eastern area consisting of Poland, Russia, and the Ukraine, rather than an extension of the western province."[72] Koslowski discusses a number of caves (4) in southern Poland, in the vicinity of Krakow, which are suggested as of early Fourth glacial date.[73] Both Mammuthöhle and Galoska contain a form of "Mousterian," whereas Okiennik and Ciemna contain "Micoquian" bifaces (although McBurney notes that if the implements from the former were found in Bavaria, they would be classed as "Mousterian"). As Zotz points out, both Galoska and Ciemna are poorly dated and may well be later than the first advance of Würm.[74] Further north, Poland suffered extensive glaciation, and no human occupation is apparent. These sites are within the permafrost-tundra zone and one can rightfully suspect human occupation so far north during a glacial. None are well stratified nor were they properly excavated, so that there is still a question as to their correct chronological position.

Babor fails to mention "Mousterian" from Czechoslovakia at this time although Breuil claimed "Mousterian" implements associated with cold fauna from the Moravian sites of Šipka and Čertovadirá (5).[75] At one time Zotz listed these sites as "Uraurignacian" (as well as the two sites, Bachofenhöhle and Byčiskala).[76] The latter two are poorly dated[77] and their position remains uncertain although the industry is probably a form of "Mousterian." Although Šipka and Čertovadirá may be early Fourth glacial, only the fauna offers evidence to suggest that this is the case. It is interesting to note that if so, the

72 Benet-Tygel, 1944.
73 Koslowski, 1924.
74 Zotz, 1951.
75 Babor, 1927; Breuil, 1925.
76 Zotz, 1941.
77 Cf. Zotz, 1951.

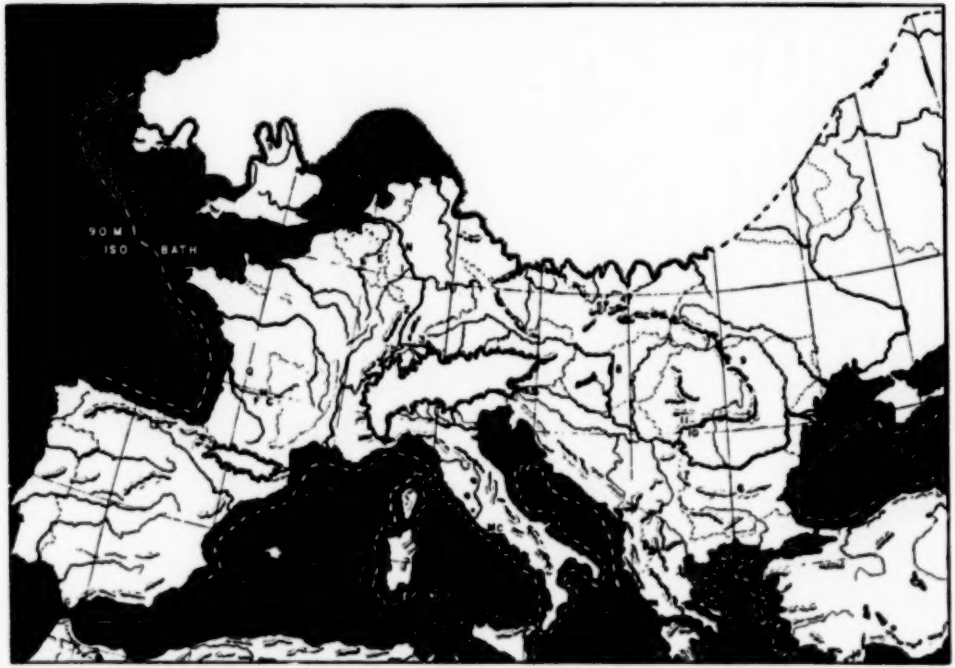

FIG. 4. Human occupation in the early Fourth Glacial of Europe. ("Classic Neandertal" skeletal remains: N—Neandertal, S—Spy, E—Engis, Q—La Quina, M—Le Moustier, F—La Ferrassie, C—La Chapelle, G—Gibraltar, MC—Monte Circeo. Numbers indicate archaeological evidence on human distribution; for key see discussion in the text.)

Šipka mandible fragment is more like that of anatomically modern man than it is "Neandertaloid" (the same may be said of the Ochos mandible from Schwedentisch grotto). A number of "Mousterian" implements are reported from Predmost but the dating is uncertain (perhaps late Third interglacial).[78]

Although "Mousterian" cleavers have been reported associated with a cold Fourth glacial fauna from Gudenus cave near Krems (on the Danube), lower Austria, there is no certainty as to the dating. Zotz notes this as a "Micoquian" industry but points out lack of any evidence for dating the material since the fauna is mixed and stratigraphy lacking.[79] No "Mousterian" or "Micoquian" implements are known from Switzerland at this time, since the area was heavily glaciated.[80]

To the southeast in Bulgaria, on the northern flank of the Balkan mountains, "Mousterian" flake implements are known from Bacho Kiro (6) and perhaps from Mirizlivka (7), near the Yugoslavian border.[81] In Hungary, at Tata (near Budapest, right bank of the Danube), Breuil noted "upper Mousterian" artifacts in a loess layer with woolly mammoth and woolly rhino fauna;[82] the lowest level of the Kiskevely cave contains similar implements which are perhaps contemporary (8). Brodar records no "Mousterian" artifacts from this time in Yugoslavia although the lack may be merely the result of the few caves excavated.[83] Nicolaèscu-Plopşor, in his survey of the Roumanian Paleolithic, notes implements of a "Mousterian facies" with cold fauna at the site of Ripiceni (9), Moldavia and an "evolved Mousterian" in a cave at Baia-de-Fier in Oltenia (10).[84] The two caves of Fedri and Ohaba-Ponor in Transylvania (11) provide good faunal and stratigraphic evidence for dating the "Mousterian" implements found there.

There is far better representation of the earlier "warm Mousterian" through the central European region, as well as accurately dated remains of an "early Neandertal" type. Human remains of this type are known from Ehringsdorf, Thuringia, from Krapina, Croatia, probably from the Mussolini cave at Subalyuk, Hungary, and from Saccopastore, central Italy.[85] "Mousterian" artifacts are known from these sites as well as others in Switzerland (Drachenloch,

78 Zotz and Freund, 1951.
79 Zotz, 1951.
80 Tschumi, 1949.
81 Garrod, 1939.
82 Breuil, 1923.
83 Brodar, 1938.
84 Nicolaèscu-Plopşor, 1935-36.
85 Weidenreich, 1928; Kramberger, 1906; Mottl, 1938; Blanc, 1948.

Wildkirchli, Wildenmannisloch), and from a number of sites in western, central, and southern Germany.[86]

It is necessary to note "the contrast between the maritime plain of western Europe and the much more broken topography forming the habitat of the suggested eastern variant of the Mousterian," with "discontinuous areas of plain separated by considerable mountain chains."[87] During a glaciation, the central European area of Germany would have been uncongenial and inhospitable; in the south, however, in valleys away from the Alpine ice, there would have been light forest cover to support a flora and fauna off which Neandertal man might have lived. The archaeological evidence indicates that there was human occupation in southwestern Germany, probably an extension of the western maritime zone. There is doubtful occupation in the southeastern region including lower Austria and probably Silesia. The central German area was unoccupied, from present evidence. The main eastern zone of occupation was undoubtedly the broad area south of the Carpathians and the eastern Alps, in the Danubian plain above the Balkans. Here under a permafrost climate supporting subarctic vegetation, there was an abundant fauna (and herbaceous flora) off which food gatherers might live, at times moving farther northward along river courses between the western Carpathians and the Austrian Alps. The physical remains of such people living here during the first stadial of the Fourth glaciation are thus far unknown (the Šipka mandibular fragment from Moravia may be from such a group) although it can be presumed that they more closely resembled the early Neandertals, and anatomically modern man, than they did the "classic Neandertals" of the western European maritime zone who were restricted to that westerly part of the continent and did not extend into central Germany or the more easterly regions.

CONCLUSIONS

The available human fossil material indicates that certain "early Neandertals" of Europe (Ehringsdorf, Krapina, Saccopastore) were most probably ancestral to the later, more peripherally western and southern "classic Neandertal" fossils.[88] The restricted distribution of the "classic Neandertal" fossils and the discontinuity of archaeological artifacts as well, suggests that a considerable degree of isolation existed between human biotopes in western and eastern Europe. In a glacial period, the climatic severity of the central European area

86 Cf. Andree, 1939; Zotz, 1951.
87 McBurney, 1950.
88 Cf. Howell, 1951.

during much of the year would serve as an effective barrier to movements between the more western maritime zone and the southwestern subarctic forest zone. Although extensions of these areas into the wooded regions just north of the Alpine ice may very well have occurred, the frequency of contact between eastern gatherers moving westward, and western gatherers moving eastward, was undoubtedly low.

The extent of isolation for human groups is of course a cultural factor, and in attempting to weigh the cultural development of the prehistoric "classic Neandertals" one is on extremely shaky ground because of the incompleteness of the archaeological record. It is, however, probably safe to regard this group as primarily food-gatherers with limited emphasis on hunting, especially of the larger herbivores; the level of subsistence being not too different then from that of certain native North American groups in the northern Intermontane area between the Cascades and the Rocky Mountains. Steward[89] has clearly shown the degree of adaptation of such groups as the Shoshoni to the rigorous intermontane environment and his general conclusions are probably applicable to the "classic Neandertals" as well, especially since the latter were considerably more culturally destitute, from all available evidence.

Specialized devices for hunting (like the atlatl or the bow and arrow) and clothing (other than simple robes of small mammal skins, perhaps) were most probably absent. Such groups were thus restricted to more genial climatic areas where there were more abundant plants, berries, nuts, and small forest animals (with deer and occasional larger herbivorous forms) off which they might live, and in a habitat which offered protection from the weather. In fact, the evidence now at hand would probably tend to support the suggestion that man was for a long period a tropical or sub-tropical animal.[90] Further, that prior to the advent of anatomically modern man and the rather elaborate cultural complexes which the artifactual record suggests for the Upper Paleolithic, he was most likely poorly prepared to cope with extreme climatic conditions such as occurred cyclically in the Eurasiatic continent during the Pleistocene.

It is necessary to emphasize the extreme difficulty of living in heavily snowed-in tundra areas without proper clothing and equipment. During the long winter months there is lack of game and vegetation in such an area. With the spring thaw, rivers become blocked with ice and the region becomes a true morass with streams overflowing their banks onto low areas. Travel is at such times nearly impossible. On the other hand, the periodic shifting of such animals as reindeer

89 Steward, 1938.
90 Cf. Bates, 1952.

northward into barren-ground areas during summer is an important factor in the life of hunting peoples.[91] During the height of the summer, however, flies and other insects make such areas difficult to occupy even for animals like the caribou. Clark has recently brought out the importance of reindeer distribution to the area occupied by Magdalenian peoples in Europe and has shown the relationship between the distribution of barbed harpoon heads and the southern extent of the reindeer.[92] The northward movement of such animals in large numbers in early spring suggests that such people as the "classic Neandertals" may well have shifted from their own southerly wintering grounds in quest of game. The innate curiosity of the animals and the fact that females are heavy with fawn make killing at this time a fairly easy matter, even with such simple weapons as wooden spears. Other arctic animals probably also moved north for summer grazing, so that brief shifts in "classic Neandertal" bands undoubtedly took place as they did with the later reindeer-hunters of the Upper Paleolithic.

Broad areas of Europe and Asia during the glacial periods, though tolerable for specially adapted arctic birds and mammals, were ill-fitted for permanent human habitation. Regions such as western and southern Europe, which were a continuous part of the temperate-subtropical continent during the interglacial periods, became, during the glacial advances, peripheral refuge areas where, though climatic conditions may have not been too congenial, there was nevertheless loss of contact with other human groups because of the surrounding climatic barriers.

The effects of such a period of prolonged isolation (at least some *thousands* of years), because of the severe climatic barriers to continuous distribution and migratory movement, will be peculiar evolutionary developments as indicated by abundant neozoölogical evidence. Gene flow is restricted to interchange between the interbreeding groups within the limited habitat and no transference of genes (or relatively none, which is the same thing) outside the biotope. Even within the habitat, gene interchange is limited to those groups contiguous with one another since most primitive food-gathering bands appear to have clearly defined occupation areas and only periodically join for purposes of communally gathering certain types of food or for carrying out religious or social activities.[93] Even within the western and southern areas of "classic Neandertal" occupation, there were undoubtedly regional distinctions as a result of various physiographic factors, although it is impossible to recognize such and they must be inferred

91 Cf. Mowat's (1952) excellent treatment of the subject among the Inland Eskimo.
92 Clark, 1952.
93 Cf. Steward, 1938.

from ethnographic data from other food-gathering populations. Because of the intergroup relations, however, genes are transferred throughout the population of the biotope for the most part, so that the concept of "classic Neandertal" for the area is a valid one. Through the even distribution of the genes, all members of the isolate share common genes and gene complexes; there is thus a degree of homogeneity obtained, like that of inbreeding, which would be otherwise impossible if there were widespread transference over large areas between migratory groups.

In small populations, such as those found among living food-gatherers, it is possible for certain mutant genes, or combinations of genes, to survive and be maintained. In fact, "the smaller the effective population size, the greater are random variations in gene frequencies, and the less effective become weak selection pressures. In small populations, alleles favored by selection may be lost and the less favored ones may reach fixation."[94] This phenomenon of genetic drift (Sewall Wright effect) may produce divergence which is non-adaptive (because of neutral genes); on the other hand, however, "although spread by drift and not by selection, [a mutant] may chance to improve the genotype and in fact be adaptive."[95] Differentiation will also take place through recombination, or the rearrangement of genes present beforehand in the genotypes.

Actually so little is known of the significance of "racial" characteristics in living populations that any suggestions as to the adaptive or non-adaptive nature of "classic Neandertal" morphology can only be of uncertain validity at the present time. The morphology of this group is, to be sure, a distinctive and peculiar one, so that it is possible to recognize the group as a geographically localized racial entity, the result of prolonged isolation during the first stadial of the Fourth glaciation. The actual mechanism of this differentiation may have been drift. On the other hand, selection is likely to have played a part, perhaps a major one, since the environment was fairly demanding of groups at a low cultural level and the difficulties encountered in the food quest undoubtedly not only restricted population size but served effectively to allow only those individuals best suited and adapted to such a habitat to survive and procreate.

Birdsell has recently discussed what he regards as drift phenomena in certain aboriginal Australian tribes, especially with regard to blood groups.[96] Although drift has undoubtedly played an important role at times in the evolutionary

94 Dobzhansky, 1951.
95 Carter, 1951.
96 Birdsell, 1950.

history of man, at the food-gathering level selection is undoubtedly very important. Since, however, practically nothing is known of the adaptive nature of morphological traits in man, one is too easily led to the conclusion that certain phenomena may be the result of drift and such an explanation allowed to suffice. The writer is convinced that, at least in the structure of the "classic Neandertal" facial skeleton and cranial base, selective forces have been the major contributing evolutionary factor at work, genetic drift having been of minor or negligible import. Research now being carried on by N. C. Tappen on the structure of the face, and by the writer on the cranial base, promises to provide clarification of this important point.

The disappearance of "classic Neandertal" remains from the fossil record following the recession of the first phase of the Last glaciation remains a problem. It has been suggested that with glacial retreat, western Europe was occupied for the first time by blade-using, anatomically modern man, "classic Neandertal" forms having either previously become extinct or being extinguished by the newcomers. There is little evidence, one way or the other, to settle the question at present. The archaeological record indicates, however, that the first western European blade-tool industry occurs during the first interstadial of the Fourth glacial and subsequent to this, abundant remains of anatomically modern type (including Combe Capelle, the Grimaldi "negroids," and many specimens of so-called Cromagnon man).

If one assumes, however, that in western Europe the "Mousterian" industry was a product of these "classic Neandertals" (and earlier Neandertaloid forms), then one is almost forced to the conclusion that in places, at least, Neandertals may well have persisted after the retreat of the first phase of the Last glaciation. Zeuner notes the persistence of the "Mousterian" industry into the first interstadial of the Fourth glacial in parts of France, East Anglia, the Italo-French Riviera, and possibly the Pontine Marsh area of central Italy (western coast, south of Rome).[97] Considerably more archaeological, as well as paleontological, evidence is needed before the reason for the disappearance can certainly be settled.

The evidence available at present indicates that the "classic Neandertals" were isolated survivors of a formerly widespread "Neandertaloid" group of people. Occurring widely over portions of southern Europe (Saccopastore), central Europe (Ehringsdorf) and southeastern Europe (Krapina) as well as in the

97 Zeuner, 1950.

middle East (Tabūn, Galilee, Skhūl?, and Kafzeh)[98] during late Third interglacial time, those in the western periphery of Europe became progressively isolated with the onset of arctic conditions heralding the first advance of the Fourth glaciation. Through the action of selection, these forms became adapted to the periglacial biotope of western Europe. With the retreat of the first phase of this glacial, more genial climatic conditions were established in Europe and the western areas were open for occupation by anatomically modern man. Whether the "classic Neandertals" were already extinct at the time of this new peopling, or whether they were extinguished by, or hybridized with these invaders, remains a moot point at present.

The lack of "classic Neandertal" characters in these Upper Paleolithic invaders make hybridization doubtful, as does the fact that remains of the two peoples living together have never been found. On the other hand, "Mousterian" artifacts still occur in some places during early blade-tool times, and thus, "classic Neandertal" persistence in certain isolated refuge areas remains a distinct possibility.

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98 Data from such Near Eastern sites as 'Ksar Akil, Lebanon (Ewing, 1947; Wright, 1951), Yabrud, Syria (Rust, 1950), and Djebel Qafzeh and Oumm Qatafa, Palestine (Neuville et al., 1951) suggests that revision of chronology is necessary with respect to the Mt Carmel population (Garrod and Bate, 1937). The writer regards the Tabūn human remains as most probably early Würm, and those from Skhūl as slightly later. This differs from my earlier statement (Howell, 1951) but still does not affect the argument presented at that time on the phylogenetic position of those hominids.

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CHANGING FOLK BELIEFS AND THE RELATIVITY OF EMPIRICAL KNOWLEDGE[1]

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ONE of the most fascinating of all the acculturation problems in Latin America today is that resulting from the contact between folk and modern medicine, for here it is possible to study not only the conflict between two systems of knowledge but also the differences between those two polarities we call magic and science. We are generally inclined to think of backward peoples as being more concerned with the supernatural and with magic than ourselves, a circumstance which led Levy-Bruhl, for instance, to consider primitive man to be pre-logical in his thinking. Recently, in his search for an "objective" criterion of progress (which he defines as the "advance to something better"), Kroeber has suggested that the advancement of a culture can be determined by the degree to which it has disengaged itself from reliance on magic and superstition. Retarded peoples, he says, place greater emphasis on phenomena that have only mental or subjective existence and thereby tend to reward certain types of psychotic and neurotic behavior that more advanced groups would consider pathological.[2]

More recently, Tax has said that although primitives reach conclusions through the same logical processes that we do, they reason from premises which differ from ours according to their "content of cultural experience." He therefore proposes a cross-cultural distinction between "knowledge" and "ignorance" by defining knowledge as "any item of information that is derived from the scientific interrelating of sense-perceived phenomena" and which accumulates through increased intercultural contacts, literacy, improved technology, increased division of labor, and the greater secularization of society. Furthermore, he equates "rational thinking" with "knowledge of the world of nature and of man"

1 The field data on which this paper is based were collected by the author for the Institute of Inter-American Affairs. I wish to acknowledge my gratitude to Drs Gomez de la Torre, Wilson Salazar, and Julio Falcony of the *Servicio Cooperativo Interamericano de Salud Publica* in Ecuador for their assistance in arranging for and administering the tests on folk beliefs and to Aníbal Buitrón for his assistance in collecting public opinions on the maternity hospital in Quito. Special thanks are due to Mr Preston Blanks, Chief of Field Party, whose coöperation made this study possible.

2 A. L. Kroeber, *Anthropology* (New York, 1948), pp. 296-300.

provided by the "scientific method" while "irrational thinking" he equates with "ignorance," the state in which mankind began.[3] Pointing out that "experimental verification" at best yields only "a statement of probability based on a correlation," Rowe accuses Tax of setting up universal cultural values on an ethnocentric basis, of assuming "absolute truth" and of suggesting a unilinear evolution of knowledge whose furthest development to date is represented by our own culture.[4]

To what extent can we truthfully say that backward peoples think differently from ourselves, that their knowledge and beliefs are less empirical, that a transition from magic to science represents progress or that an evolutionary concept of knowledge must be unilinear and ethnocentric? In the pages which follow we shall endeavor to throw more light on these questions through a study of folk vs. modern medicine in Ecuador. Special attention will be given to situations where folk practices and beliefs are being challenged by their modern counterparts, for the type of acceptances and resistances we encounter in these situations can help us to evaluate whatever differences may exist between folk and modern thought.

Our study will be divided into three sections. The first will present a summary sketch of the relevant data on folk medical beliefs and practices collected in the poorer districts of Quito. Although our material on the subject from the towns of Tulcán and Esmeraldas was in general very similar to that from Quito, it is not included in this section in order to avoid the geographical comparisons that would be necessary. Field methods used for the collection of this data included interviewing, observation of the curing procedures of folk practitioners, and a statistical study of the sale of remedial herbs in the central public market of Quito. Secondly, we shall describe special situations of change in Quito, Tulcán, and Esmeraldas. Methods here included interviewing, small public opinion surveys, and the testing of school children, nursing students, and nurses' aids. The final section will be devoted to a theoretical analysis of our data in relation to the questions we have asked.

FOLK MEDICINE

Beginning with the folk concepts of disease etiology, we may consider first those illnesses attributed to contagion, although the folk concept of contagion is different from the modern one. The major folk explanation is concerned with

3 Sol Tax, *Animistic and Rational Thought* (Kroeber Anthropological Society Papers, no. 2, pp. 1-5, 1950).

4 John Howland Rowe, *Thoughts on Knowledge and Ignorance* (Kroeber Anthropological Society Papers, no. 2, pp. 6-8, 1950).

the fear of bad body humor. This is most commonly described as due to lack of personal cleanliness. It is a substance which exudes with perspiration, and if one does not bathe frequently it may re-enter the pores and infect the blood. Not only is it a source of auto-infection, but it may also be passed between persons. Thus it may cause such ailments as skin diseases, infected wounds, and syphilis. Close contact, sexual relations, or seats still warm from a previous occupant are means by which it may pass from one individual to another.

Next we may consider those folk illnesses attributed to "mechanical" etiologies. By mechanical we refer to such things as temperature change, harmful foods, fatigue, and body blows. Of all the mechanical causes those concerning temperature are probably the most important for the folk. Exposure to cold air when overheated is considered very dangerous, and being caught in a thunder shower when working in the fields or drinking cold water when perspiring are other variants. Symptoms may include dysentery, menstrual and postnatal cramps, pneumonia, urinary difficulties, rheumatism, measles, partial paralysis, and malaria. Body heat may cause skin diseases, and heat generated in the body by coughing may lead to angina. Certain foods are dangerous by reason of their "heaviness," "sourness," "acidity," or inherent "coldness." Some foods stick to the stomach and others lead to an overabundance of body bile which is related to liver trouble. Any cooked food which is left to stand acquires a quality of coldness which is especially feared. Fatigue from hard work and insufficient food leads to "liver" and "kidney" ailments as well as skin infections and "inflamed uterus." Body blows can cause meningitis and tumors. Diarrhea with fever is said invariably to accompany the process of teething. For some informants, diarrhea at this age is simply a kind of "natural" consequence. Others give a psychological explanation. They say that because the child is irritated he develops "anger" sickness.

There are several illnesses that the folk ascribe to psychological causes. Among these is anger sickness due to quarrels, jealousy, etc. This may result in such symptoms as vomiting, diarrhea, fever, inability to walk, depression, "fits," and "palpitation of the stomach." An unfilled desire such as a sexual desire, a food craving of a pregnant woman, or the desire of a small child to possess a toy or candy may result in a variety of maladies. "Fits," syphilis, and urinary difficulties are examples. Sadness due to a personal loss (loss of a loved one, money, property, etc.) may also cause "fits" as well as palpitation of the heart, fever, lack of appetite, severe headaches, loss of consciousness, etc.

Among the supernatural ailments fright and malevolent air are the two most frequent causes and the victims are predominantly children. The stimulus

for a fright may be anything as natural as a sudden fall or as supernatural as an encounter with a ghost or a spirit. For many individuals there seems to be a definite feeling that the fright produces soul-loss. Malevolent air may be contracted by a small child if he is taken to a cemetery, too close to a corpse, to solitary places in the mountains, down into mountain canyons, or is exposed to a rainbow or night air. Another prevalent childhood illness, especially among pretty children, is that due to evil eye, the malevolent glance of certain adults who have "electrical" or "strong" eyes. For adults witchcraft is the primary supernatural cause of illness. The methods of the witch include magical poisoning of the victim's food and imitative magic using dolls to represent the victim. Symptoms of all ailments having supernatural etiologies are so generalized that they could point to almost any malady in a modern classification. However, among those listed for children, informants always included fever, vomiting, and diarrhea.

The pains frequently experienced by women after parturition are also attributed to supernatural causes, for they are considered likely to occur if the placenta is discarded in such a way that it comes in contact with water or cold air or is eaten by dogs. The placenta should be buried in a dry, safe place such as the ground beneath the kitchen hearth.

Folk preventive measures may be classified as either mechanical or supernatural. The "contagious" diseases are prevented by such mechanical means as bathing and by avoiding warm seats. The "mechanical" illnesses can be prevented by avoiding sudden changes of temperature, by regularly taking cathartics to keep the stomach clean, etc. Psychological illnesses are considered almost impossible to prevent since they are brought on by circumstances which the individual cannot control or readily avoid. Supernatural ailments may be prevented by both mechanical and supernatural means. Avoidance of those situations in which a child is susceptible to supernatural infections could be considered mechanical. But since avoidance is not always possible, the supernatural means of prevention predominate. Wearing certain objects on one's person is the most common type of preventive measure. The beak of a parrot or the wool of a llama will protect against witchcraft. A rosary around the neck of a child will guard it against malevolent air while crossing a canyon. Steel rings protect their wearers against both malevolent air and witchcraft, while a red ribbon will protect a child against the evil eye.

Very often diagnosis is made by reflecting over preceding events. Was the sick person recently exposed to a sudden change of temperature? Did he enter a canyon where there might have been spirits? Did he make an enemy who might have had him bewitched? Professional curers diagnose largely by magical

means such as measurement of the patient with red ribbons (Esmeraldas), cracking the patient's back, or throwing corn grains in the embers of a fire. Some of these magical methods of diagnosis are curative as well.

The predominating type of folk remedy consists of herbal solutions and broths, the cathartics being the most popular of all. Calmative herbal broths employed for stopping diarrhea and vomiting as well as sudorific herbal broths are, like the cathartics, usually taken orally while others are administered in the form of enemas and vaginal douches. Among the external remedies, herbal poultices are very often used. On the supernatural side, cupping may be employed to extract malevolent air. But the most popular remedy for ailments with supernatural etiologies is that of "cleaning" the patient with special plants, eggs, or guinea pigs. The "cleaning" process consists of slowly rubbing the remedy over the patient's body until the illness is drawn into it. Religion also plays an important part in curing in the form of vows to saints or praying "Our Father" and Credos in magical sets of three, etc.

In a sense, every adult is a medical specialist. The folk share a common knowledge concerning the diagnosis, classification, and treatment of symptoms and are most likely to consult a curer only when their own household remedies fail. If a supernatural cause is suspected, however, only the curer is considered capable of properly administering the "cleaning" treatment.

An attempt was made to gauge the incidence of folk illnesses and the extent to which folk remedies are used by observing purchases of herbs for a period of several days at the central public market in Quito. These remedies were sought for practically every kind of malady imaginable, but a few illnesses had a considerably higher incidence than others. The six illnesses of highest incidence were malevolent air (nearly all children), "cough" (children), fright sickness (children), menstrual difficulties (mostly cases of retarded flow, and the cathartics purchased may well have abortive effects), urinary difficulties (mostly adults with symptoms of blood and pain), and bewitchment (all adults). If the period at which the investigation was made is representative of yearly conditions, at least 180,000 purchases are made each year at this one market alone, a figure almost as great as that of the population of Quito. One fifth of all the purchases were for supernatural ailments and consisted of herbs with magical "cleaning" properties.

CHANGE AND CONFLICT

Some general knowledge of the modern concept of microbial infection was found to exist in all locales where studies were made. However, even though school children in particular were often capable of giving good explanations of

modern concepts of disease etiology, they were also capable of giving just as good explanations of the traditional concepts. To check the impressions obtained from interview material, tests were given to grammar school children in Quito. Their responses showed they knew and adhered closely to the folk beliefs which had previously been uncovered in the personal interviews. On questions concerning the doctor's ability to cure those ailments whose etiologies we have classified as supernatural, the answers were almost unanimously negative. Since doctors do not study these maladies, they do not know the curing techniques appropriate for them. Answers to a question on the nature of the microbe and its dangers indicated some understanding of it. It was defined as a tiny animal that could only be seen with a microscope, that lived in unclean food and water, etc. and was an agent of disease. But the diseases actually associated with the microbe were those with modern names such as tuberculosis, typhoid, and whooping cough. When asked if microbes could be the agents of such ailments as fright sickness, malevolent air, and bewitchment, the responses were again almost entirely negative. If a microbe can be seen only through a microscope, how can it frighten anyone? Such illnesses are caused by spirits, cold winds, and evil persons and are not the results of contagion or infection.

Interview material showed that in general the illnesses with supernatural etiologies are treated by folk specialists, while others such as infected wounds, measles, anger sickness, and skin infections are treated at home. However, even such folk specialists as curers and professional herbalists will agree that the doctor is the one best qualified to treat such ailments as diphtheria, tuberculosis, venereal disease, and appendicitis. In many cases the informant will admit that he goes to the doctor only when his own remedies or those of the curer have failed. Judging from the symptoms given for many of the folk etiologies, it would seem that even though the folk have more confidence in the doctor as a medical practitioner for many maladies with modern names, they do not always classify their symptoms according to those names until the doctor has been consulted at an advanced stage.

In another set of tests conducted in a Quito grammar school, the children were given a list of twenty-two illnesses and asked to check the ones they would treat at home, those they would have treated by folk curers, and those they would have treated by a doctor. The list included folk and modern illnesses and in no case was there a 100 percent response in favor of any particular means of treatment. Responses in favor of the doctor ranged from 96 percent in the case of tuberculosis to only 2 percent in the case of fright sickness. All the supernatural ailments as well as such maladies as skin infections, infected wounds, and

diarrhea fell among those for which more than 50 percent of the subjects would seek a curer or treat at home. For ailments such as bronchitis, typhoid, paralysis, whooping cough, and malaria more than 50 percent preferred the doctor. The same children were also given a list of causes and asked to match them with these illnesses. The most frequently indicated cause of illness was microbes, which gained its weight from those diseases on the list bearing modern names. In all, however, it accounted for only 19 percent of the total number of responses. Folk causes were predominantly given for illnesses treated at home or by a curer. The most significant fact was that even in the case of those illnesses for which medical treatment was favored, supernatural causes were in some cases predominant.

From a consideration of our data so far, it would appear that the folk look up to the doctor for his ability to cure serious illnesses for which their own remedies are less likely to be efficacious, independently of whether or not they understand or believe in his explanation of causes. Their acceptance of the doctor rests primarily upon empirical observation and experience. To test this further, we will now compare the results of investigations at Esmeraldas and Tulcán. In the Negro coastal area about Esmeraldas, yaws was a prevalent disease until an extensive campaign of injecting penicillin was inaugurated. Doctors who served in the campaign encountered considerable public resistance at first. By the time of my visit, however, yaws had been pretty well brought under control, and if there ever had been any resistance to the program it was certainly no longer in evidence. Although investigations in Esmeraldas showed a strong adherence to folk medical beliefs and treatment, house to house interviews disclosed nothing but praise for the yaws campaign and a complete conviction that yaws was a disease which the doctor alone was capable of curing. This attitude was shared even by local curers. As some informants expressed it, you cannot deny what you see with your own eyes.

The situation in the highland town of Tulcán was somewhat different. A water purification system had just been installed and house to house visits revealed that while the folk were very much in favor of the purified water, their reasons had nothing to do with disease prevention. Previously they had had to let their water stand in buckets until the dirt settled; now it was possible to use it directly from the public taps. For the most part, however, they did not believe that the former water had caused them any harm. Interviews with informants and a native curer disclosed that the symptoms of intestinal infection were being attributed to malevolent air and fright sickness and were being treated by magical means.

The data from Esmeraldas and Tulcán give further indication that the acceptance of the doctor is based primarily on empirical knowledge and observation. The campaign at Esmeraldas was curative with spectacular and observable results. At Tulcán, on the other hand, the program was one of preventive medicine based on modern concepts of disease etiology that were not as easy to grasp on a purely empirical basis. Perhaps a noticeable change in the incidence of certain syndromes at Tulcán will lead the folk to an empirical judgment in favor of modern preventive medicine. But it seems just as likely that their failure to grasp the modern concepts of disease etiology will result in the contamination of food and water through other means, thus preventing any noticeable change in the incidence of syndromes.

Let us now turn back to Quito and the new charity maternity hospital, Isidro Ayora, where attendance runs as high as sixty percent of the total number of Quito births. On interviewing mothers concerning the hospital it was found that some profound changes were transpiring in their beliefs. Such hospital practices as flushing placentas down the drain and requiring patients to leave in five days (according to tradition a mother should remain in bed for fifteen and in the house for forty) were causes for alarm, but all women who had attended the hospital stated that their fears were evidently groundless since no ill effects had resulted. Arguments overheard between mothers who had attended the hospital and mothers who had not indicated that those who have attended are "selling" the hospital to others and arguing against their own former folk beliefs. The fact that all services at the hospital are free and that many mothers are recommending it seem to be the primary motivating factors for attendance in the case of young mothers going for the first time.

However, not all goes smoothly at the new maternity hospital. Because the modern concepts of medicine are not understood, many of the preventive measures find little acceptance. Once inside the hospital, mothers for the most part refuse to be bathed after parturition, to eat certain foods which they consider harmful, or to go without their post-natal laxative. Those hospital practices in conflict with their beliefs but over which they have no control are effecting changes in those same beliefs on an empirical basis. Those beliefs in conflict with hospital practice that the mothers can control by fits of crying or threats to leave are persisting and disrupting the hospital routine.

As far as tradition is concerned, we may say that the folk beliefs in themselves are offering no resistance to modern medical practices in so far as those practices may be judged by the folk on an empirical basis. Preventive medicine,

however, is being resisted because its comprehension is largely at a theoretical level that does not readily lend itself to empirical observation. Let us now consider the effect produced on the folk beliefs by higher education and special theoretical training.

A test on medical beliefs was given to fifty-five nurses' aids at the maternity hospital Isidro Ayora and an equal number of student nurses at the national school of nursing. While the nurses' aids had had only grammar school education, the student nurses were all high school graduates.

By comparing, first, the responses of the nurses' aids (who had had little additional theoretical training) with interview material and the tests of grammar school children, it is possible to make some estimate of the extent to which hospital life has affected their beliefs. In doing so we find that the only clear distinction pertains to the more supernatural ideas. Causes such as witchcraft and improper disposal of the placenta, preventive measures such as red ribbons and religious medals, and such treatments as "cleaning" and praying were no longer important to them. However, while beginning nursing students, with the advantage of a high school education, were much more inclined to affirm their disbelief on these questions, many of the nurses' aids refused to answer them. Third-year nurses showed the most remarkable change, for over seventy-five percent stated disbelief. Third-year nurses also distinguished themselves from all the rest by a forty-five to sixty-one percent greater frequency of stated disbelief concerning postnatal food taboos, "natural" diarrhea as an accompaniment of teething, fright sickness, malevolent air, and sickness due to desire. The most persistent beliefs among all groups were those concerning anger and sadness, body humors, body biles, temperature changes, and the preventive and curative use of laxatives. (Among the better educated groups very innocent symptoms were attributed to the two psychological etiologies, anger and sadness.)

When one considers that none of the subjects had ever had any course which directly discussed and attacked their folk beliefs, the results indicate the extent to which they themselves have reconciled the old and the new. When asked what they thought accounted for their greater degree of disbelief in folk concepts, the third-year nurses attributed the change to a course in obstetrics that the second-year nurses had only just begun. General education as well as specialized work which brings the person in contact with modern practices correlates with greater disbelief in the more supernatural and magical concepts. Contact with modern practices does not, however, have as marked an effect on changing those beliefs concerning cause and prevention that we have called "mechanical" as

do more advanced theoretical explanations. Those beliefs that are most persistent under any circumstances are those whose psychological or mechanical explanations most closely approximate the modern.

The marked influence of higher theoretical training may well correlate with greater relegation of authority to the modern specialist. In response to a question that asked the subjects to state what they would do in case a mother in a maternity hospital requested a postnatal laxative, the third-year nurses were far more apt to leave the decision to the doctor, while the nurses' aids were inclined to agree with the patient and accept the responsibility for administering it. In general, the folk seem much more prone to consider themselves self-sufficient in medical treatment and diagnosis than the average North American. Where the North American may become nervous at the slightest symptom and run to a doctor, the folk immediately classify or diagnose the symptoms according to their own extensive body of knowledge and beliefs and attempt to cure them by themselves. In the grammar school class that was asked to designate which illnesses could be cured at home and which by a curer or doctor, the boys showed a greater dependence on the specialist whether he be a curer or a doctor while the girls had more faith in the home remedies they had learned from their mothers.

Social factors are also important in the process of acculturation from folk to modern medicine. Among white collar and professional classes in Quito a feeling exists that only people of inferior status use herbal remedies and go to folk curers. The "better" people are supposed to rely on drugstore remedies and doctors. A test given to one hundred boys in a public school attended by children of the white collar class showed a much greater reliance on the drugstore and the doctor than the children of the poorer districts. However, in their concepts of disease etiology the two groups were much more closely allied. It would seem that the acceptance of the doctor and his remedies is related in some degree to prestige, independently of an understanding of modern etiology.

In Bogotá, Colombia, where the author has made similar investigations, social factors play an important part in public resistance to free health centers. Although the folk beliefs themselves do not appear as strong nor as well organized as in Quito, there is greater hostility toward modern medicine in the poorer districts. Colombia has been affected by a tremendous urbanization movement that has disrupted the old class system. "Good family" is not the important social criterion it was formerly, while acquired economic status is becoming more and more important. The cities have been flooded by rural immigrants who no longer classify themselves or one another according to a traditional system. As a result

of the competition to rise socially, individuals with some small position of authority press their weight on others to force a recognition of the status they wish to have associated with that authority. Thus, the nurses and doctors employed in the government health centers are often overbearing in their treatment of the public. In resentment, the Bogotá folk freely voice their hostility toward modern medicine. Common household items are books on folk remedies written with overtones of spiritualism by nationally famous curers.

The social factors are related to economic ones. If the "better" people in Quito rely more on the doctor despite a persistence of folk beliefs among them, this is not only due to the doctor's prestige but to the fact that they are in a better position to afford him. Where modern medical therapy is free, attendance is often greater than the traffic will bear. To judge from their appearance, the great majority of the people who frequented the herbalists at the public market undoubtedly belonged to the lowest economic groups in Quito.

CONCLUSIONS

First, let us consider the problem of whether the folk can be said to think differently from ourselves or whether their knowledge is less empirical. In treating this problem we shall make use of concepts employed by Dr Hans Reichenbach in his *The Rise of Scientific Philosophy*.[5] Dr Reichenbach states that the essence of knowledge is generalization and that correct generalization is dependent upon the separation of relevant from irrelevant factors. By "relevant" he means that ". . . which must be mentioned for the generalization to be valid." Further, he considers generalization to be ". . . the very nature of explanation" which is sometimes achieved ". . . by assuming some fact that is not or cannot be observed." He gives as an example the casual explanation whereby the barking of a dog is attributed to the presence of a stranger in the vicinity of the house, an unobserved fact which is ". . . explanatory only because it shows the observed fact to be the manifestation of a general law [that] dogs bark when strangers approach. . . ." Such inductive inference, he claims, goes beyond a mere summary of previous observations and becomes an instrument for predictive knowledge. But we can never be sure that dogs will always bark at strangers. In this vein, Dr Reichenbach says, "The study of inductive inference belongs in the theory of probability, since observational facts can make a theory only probable but will never make it absolutely certain." Thus, in place of absolute knowledge

5 Berkeley, 1951. The quotations in their order of appearance are taken from pages 5-7, 229, 94, 263, 240, and 246. Italics ours.

he speaks of "probable knowledge," a term he does not find self-contradictory because it is based on "frequency interpretation" in which "degree of probability is *a matter of experience* and *not of reason*." From here we may proceed to his concept of a *posit*, ". . . a statement which we treat as true although we do not know whether it is so." Yet, "we try to select our posits in such a way that they will be true as often as possible." The reason we try is ". . . because we want to act—and he who wants to act cannot wait until the future has become observational knowledge. . . . Posits are the instruments of action where truth is not available; the justification of induction is that it is the best instrument of action known to us."

To what extent is folk belief and knowledge a result of induction or "frequency interpretation"? The material resulting from our study of changing folk beliefs in Ecuador has indicated that for the most part the changes are transpiring on just that basis. In the field of therapy, modern medicine is making demonstrations on such a scale that the folk can readily establish *posits* for future behavior in favor of modern therapeutic methods. Resistance is primarily directed at modern preventive medicine in which the generalizations and explanations involved result from frequency interpretations that are not possible under situations of casual empiricism but are dependent upon the observations of trained investigators. The communication of this type of frequency interpretation to non-investigators takes place most easily under situations of special training as we saw from our testing of nursing students.

If we consider the folk beliefs alone without reference to change, it is possible to encounter many generalizations resulting from frequency interpretations, in other words, correlations made between repetitive phenomena. The concept of bad body humor, for example, is very close to our own explanation of contagion. It involves an inductive inference based on past observations of a correlation between the onset of disease and certain situations in which contagion is likely. The explanation also assumes something that is not observable, a substance called bad body humor. However, it leads to predictions of situations in which disease can be contracted or avoided. Bodily cleanliness, for example, is a predictable preventive measure that still holds good under modern explanations of contagion although the prediction that disease may be contracted from a warm bus seat does not. The entire concept of bad body humor is a type of probable knowledge involving posits that definitely aid the folk to take action even though some of its posits are far less probable than others.

According to folk belief, cooked food that has been left to stand may acquire a dangerous quality of "coldness." Symptoms resulting from eating such food

sound remarkably like those of botulism or enterotoxin-producing staphylococci. The folk have made a correlation between illness and cooked food that has been left to stand, and their prediction that illness can be avoided by re-cooking the food and thus removing the "coldness" is valid to the extent that in practice it would lead to a detoxication of the poison. But we must note that the preventive measures of cleanliness and storage that would also help to prevent food poisoning are irrelevant to the folk in view of their theory of "coldness." However, unlike bad body humors, the "coldness" of cooked food that has been left to stand is a part of folk observation. Since food infected by staphylococci or spores of *C. botulinum* does not necessarily change in taste or smell, the modern explanation is beyond the limitations of folk experience. Therefore, we see how the validity of an inductive generalization or correlation may be independent of the validity of its *causal* explanation: a condition that is common knowledge to any statistician and the principal reason why the modern scientist has come to think in terms of probability rather than causality. The modern refinement of this folk explanation has resulted from the frequency interpretations of trained laboratory investigators who have simply *increased* the posits by which we govern our actions to avoid food poisoning.

The folk have also drawn a correlation between the symptoms of intestinal infection and the teething stage in the child's development. In this case the explanations add so little that no prediction is possible except that the sickness is inevitable. Since we know that teething children are more apt to insert objects into their mouths and thereby increase the possibility of intestinal infection (under conditions of folk life almost any object on the dirty floor), the folk correlation or generalization is both relevant and valid. However, it is a very simple empirical correlation. The correlation between the symptomatology and the many diverse activities of the teething child that would be necessary to provide a modern generalization is so complex as to make its empirical observation very difficult on a casual basis.

Let us now consider the supernatural type of explanation. Using a completely hypothetical example, we may take the case of a group of mothers who cross a canyon and stop to give their children a drink of water from the polluted stream flowing through it. Later, when the children show symptoms of intestinal infection frequently classified by the folk as malevolent air, the mothers may make a correlation between the illness and the trip through the canyon. Their correlation may be partly correct despite the irrelevance of their belief in spirits or the fact that they overlooked the relevancy of the stream. We cannot, however, use this hypothetical example as an argument that some empirical correlation invari-

ably underlies all folk beliefs. But, as the testing of the nursing students and nurses' aids would seem to indicate, situations providing greater opportunities for understanding modern frequency interpretations are more detrimental to the supernatural explanations and practices than those for which it is easier to discover some underlying empiricism.

On the side of magical treatment, it is hard to see exactly how empiricism enters into the picture at all when a curer attempts to remedy an illness with "cleaning" herbs or guinea pigs. However, we must consider the fact that not all illnesses are fatal. On the average, success is in favor of the curer. This is not so true in the case of modern classifications of illnesses by which we can appraise the relative danger of syndromes with greater exactness. But the symptoms which the folk ascribe to many of their classificatory terms and etiologies are so highly generalized that they may include both fatal and innocent maladies. As long as the law of averages works in favor of the curer, his results are an empirical demonstration that his methods, as well as the theories and explanations on which they are based, are in general valid though not infallible. Our own reasoning may work along similar lines. Recently a well-known professor of medicine from an American university admitted to me that had he been treating himself with one of the modern "miracle" drugs he would probably have credited it with his recovery from a cold that had unexpectedly disappeared without any treatment whatsoever.

In many cases of "supernatural" illnesses the etiology may be psychological. The kinds of partial paralysis that some informants gave as the symptoms suffered by close relatives who had been bewitched as well as their claims that doctors were unable to find a cause, much less a cure for these cases, sounded very much like descriptions of psychosomatic conditions. Furthermore, curers succeeded in relieving the symptoms in a single magical treatment. Again, this constituted a definite demonstration to the informants that the cure was a proof of the correct diagnosis of the cause as well as the efficacy of the treatment. The similarity between these cases and those of psychological or religious practitioners in our own society almost tempts a comparison. A large part of the influence of psychoanalytic dogma on the social sciences is undeniably due to a logic that accepts a cure as "scientific" proof of the theoretical postulates underlying the method of treatment. This writer wonders if "magic" in its broadest sense can ever be completely divorced from "science." Both provide posits for future action, both may include irrelevant correlations, and both may be based on probable knowledge resulting from frequency interpretations. However, the experiences

of the folk cannot provide them with the type or quantity of frequency interpretations that can be derived from laboratory experiences.

If it has been difficult to find any difference in kind between the thinking of backward and "civilized" peoples, to what extent are we justified in claiming a greater dependence on magic and supernatural explanations among the more backward? We feel that this question can best be answered by making use of the distinction between esoteric and exoteric population components. Few persons in the exoteric component of modern society attempt to recapitulate the discoveries of such men as Pasteur and Koch, much less view a microbe through a microscope. They accept the authorities belonging to our esoteric component who claim that microbes cause certain maladies that may therefore be prevented or cured in a certain way. During experimental health lectures given to a group of school children in Quito, the children were questioned about spirits and ghosts. None had ever seen one, but certain authorities such as curers or professional herbalists had. Thus it would appear that the greater dependence on magic and supernatural explanations among backward populations is due principally to differences between their esoteric components and ours. How can we explain this difference between the esoteric components of backward and modern populations?

The answer to our question seems obvious enough. Due to technological advances a far greater proportion of individuals in our society have been freed from direct food pursuits to enter many fields of specialization. Our technological system is therefore capable of supporting an esoteric component within our population that can devote full time to checking our casual empiricism, to expanding our frequency interpretations, and to providing us with a knowledge that is much more probable in comparison with that of backward peoples. This becomes clearer when we consider the fact that even the folk curer is usually only a part-time specialist. The author knows of young doctors who attempted to practice in rural areas and were forced to leave because even what few payments they received for their services were generally payments in produce. It is not difficult to understand why the type of "pure" research which is not even directly related to immediate needs is dependent upon a very complex technological system as compared to that of the folk. Nor is it difficult to understand why a society that cannot extend its opportunities for frequency interpretation beyond the casual empiricism offered by every-day situations will focus its interests in such a way as to reward its esoteric component for posits that appear to have a strong supernatural basis from a modern point of view. In this way, "backward" groups

may come to reward certain types of individual behavior that might be regarded as pathological when occurring among members of our society. Therefore, the difference between what we tend to think of as two extremes, magic and science, is related in large part to the limitations placed upon the esoteric component of a given population by the technological system.

The final problems mentioned in our introduction are concerned with concepts of progress and evolution. The major objections to the word "evolution" among present-day cultural anthropologists are related to the fact that early anthropologists explained all parallels between cultures as due to a unilinear type of progression, and because they reconstructed culture history by assuming that what was different from Western civilization was inferior and therefore older. Subsequent studies have not only demonstrated differential development but the cultural relativity of values. Objection has also been raised against evolutionary schemes because they did not seem as applicable to non-material aspects of culture as to those that were strictly technological. Yet, if we consider the work of three modern neo-evolutionists, we find we can apply their concepts to such a "non-material" subject matter as magic and science without repeating the mistakes of the earlier evolutionists. Leslie White has seen a progression in culture based on the energy harnessed per capita per year.[6] His evolutionism is primarily technological. While not an avowed cultural evolutionist, Carleton Coon has scaled cultures by levels of complexity on what he calls a quantitative basis. His quantification rests upon the complexity of institutions, the number of institutions to which the average individual belongs, the amount of trade, and the degree of specialization.[7] George P. Murdock, who is more concerned with accounting for similarities between cultures than with systems of progression, explains the development of parallel social systems as due to limitations in the number of variations possible.[8]

By combining the three perspectives above we may arrive at a tentative hypothesis. The amount of energy harnessed per capita per year by the technological system of a society places limitations on the size and the type of its esoteric population component as well as limitations on the range of experience available to that component as a basis for frequency interpretations. This in turn limits the range of a society's probable knowledge and the degree to which its participants can estimate the probability of their predictions. Such limitations

6 Leslie White, "Ethnological Theory" (in *Philosophy for the Future*, R. W. Sellars, ed., New York, 1949), pp. 357-384.

7 Carleton S. Coon, *A Reader in General Anthropology* (New York, 1948), pp. 612-614.

8 George P. Murdock, *Social Structure* (New York, 1949) pp. 184-259.

do not determine precise historical sequences nor precise regional variations. Within the limitations are such allowable variations as were treated in our comparison of social factors in Quito and Bogotá. Nor does this specialization necessarily imply any change from "worse" to "better." On the contrary, we can speak of an evolution from magic to science that is culturally relative. The relativity of this evolution depends upon the fact that the probability of knowledge is itself relative to the limitations inherent in a given cultural situation, and at no point in a progression is it "better" except as measured by the value system of an observer at that particular point.

Returning now to the disagreement between Tax and Rowe, we find that the conclusions of this paper are in accord with Tax's main thesis. The principal differences between the positions of Tax and the present paper are that we have made no qualitative dichotomy between "knowledge" and "ignorance," have suggested a quantitative difference in degrees of probable knowledge and have employed Reichenbach's distinction between probable knowledge and reason. Except for Tax's failure to stress the difference between rationalism and empiricism, we find nothing in his paper that would justify Rowe's criticism that he was assuming absolute truth and using universal and ethnocentric values. According to Rowe, all ethnographers are faced with ". . . the great dilemma of cultural anthropology: the dilemma of cultural objectivity." Thus, the anthropologist who is striving to be objective, that is, to keep his own cultural values out of his comparisons, has but "two obvious choices": (1) ". . . to accept, in so far as possible, the cultural values of the culture being studied . . ." or (2) ". . . to attempt to set up universal values which we can then apply to all cultures."[9] Rowe admittedly prefers the first choice and ascribes the second to Tax. However, if each culture must be appraised separately in relation to its own "values," cross-cultural comparisons are doomed to rationalistic rather than empirical treatment and cultural anthropology to an anecdotal preoccupation with the minutiae of differences. Is there really no other choice than the two Rowe offers? Is there no other way of guarding against value judgments than to adopt a different set of vəlues?

In Ecuador, the magical treatment of illnesses reaches its greatest incidence among children below the age of five, who, like the old, are considered to be weaker and more susceptible to supernatural causes. However, the illnesses from which these children actually suffer are not a peculiar figment of this culture; they are the same which may afflict children anywhere. The death rate of this

9 Rowe, *op. cit.*, p. 6.

age group, furthermore, is over fifty percent of the recorded yearly deaths in Ecuador as compared to a figure of less than ten percent in the United States. Given these two societies, each of which is endeavoring to apply its knowledge to the prevention and cure of the same illnesses, it is perfectly legitimate to state that the knowledge of the great majority of Ecuadorians with respect to childhood illnesses is *less probable* than the knowledge of North Americans. To claim their knowledge is "inferior" would depend on whether or not we consider a lower infantile death rate as "better." But the second proposition is not necessarily a corollary of the first. The first is concerned with the observation of human behavior, the second with a value judgment about that behavior.

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STUTTERING AMONG THE NORTH PACIFIC COASTAL INDIANS[1]

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THE FOLLOWING is a report and tentative interpretation of research findings of a field investigation into the incidence and cultural aspects of stuttering among a number of Indian tribes in the North Pacific coastal area of British Columbia. The project was part of a larger enterprise to gather comparative materials on deviant behavior but more specifically followed certain lines of inquiry initiated by clinical psychologists working on the problem of speech disorders in American Indian societies, who were unable to find evidence of stuttering in these groups.[2]

Our field contacts covered five reserves of Salish Indians, two Kwakiutl and one Nootka or west coast tribe, all located on Vancouver Island and the mainland of British Columbia. In addition to these contacts with the cultures *in situ* we had two Haida informants, one Tsimshian, and one Nootka who were not living in their respective areas. All informants except two spoke English and interviews were necessarily conducted in this language. In addition, our body of interview data was supplemented by information from school officials and some written materials from Indian children at the Port Alberni and Alert Bay residential schools.

In general our findings emerged as exceptional or at considerable variance from those indicating an absence of speech disorders among the American Indians. Thus we encountered: (1) both stuttering speech and persons regarded as stutterers among the contemporary coastal Indians; (2) evidence of pre-acculturational existence of this class of speech disorders; (3) well defined concepts of stuttering and stutterers in the language and culture of all the bands of which we came to have knowledge.

1 The writer is indebted to Professor Abraham M. Halpern for several very helpful suggestions and for assistance in the transcriptions in this paper.

2 Wendell Johnson, *The Indians Have No Word For it: Stuttering in Children* (Quarterly Journal of Speech, vol. 30, pp. 330-337, 1944); also, his *People in Quandaries* (New York and London, 1949), ch. 17; John Snidecor, *Why the Indian Does Not Stutter* (Quarterly Journal of Speech, vol. 33, pp. 493-495, 1947). Inquiries by an investigator of stuttering among the Navaho brought to light altogether five cases, some obviously complicated by acculturation problems. She concluded that while cases do occur in this tribe nevertheless it was quite rare (A. K. Bullen, *A Cross Cultural Approach to the Problem of Stuttering*, Child Development, vol. 16, pp. 3-7, 1945).

Since our investigation was in no sense a systematic survey, it was impossible to arrive at any measure such as a percentage or a rate of stutterers in the Indian population. However, the following facts indisputably establish the existence if not the frequency, of the speech defect in the area. In the residential school at Alert Bay two stutterers (both female) were identified out of a total enrollment of 220. Also two were reported and described for us at the residential school at Port Alberni where the enrollment reaches 240. At the Cowichan day school, with a student group of 110, no stutterers were known to the teachers but "quite a few" had trouble with "g" sounds, which caused them considerable shame and embarrassment.

All of our Indian informants (ten) with the exception of one[3] whom we queried on the subject were acquainted with at least one or two cases of stuttering, and several gave clear statements that the disorder was commonplace among their people, especially the two Nootka informants. Information from the Homalthko Salish band testified to the presence of the disorder in several families. In one of these there are two brothers who stuttered as children and who do so currently, although one of them experiences his broken speech only when drunk. There is another adult stutterer in a second family, and in still a third family there is an adult who stuttered as a child but was later "cured." This man's deceased grandfather also stuttered, both as a child and when grown to maturity. The population of this band fluctuates around one hundred fifty. In a closely related band, the Tlahoose, we talked directly with a man and his sister who had been very bad stutterers as children.

Apart from these we were able to interview two Indians who were currently stutterers, one a Tsimshian youth of twenty years, the other a young Salish boy. A brief case history of the latter discloses the nature of the stuttering.

Stanley Jones was born fourteen years ago at Sliammon, a reserve located a few miles outside of Powell River, British Columbia. He is an illegitimate child, having three younger siblings and one older sister. The mother apparently was sexually promiscuous when younger and at the present time is one of those on the reserve who make and get drunk on home brew. At times Stanley has been left in the care of his older, married sister when his father and mother move away to live at logging camps. This sister is drunken and sexually promiscuous, and regards Stanley as incorrigible. Stanley has had tuberculosis and bears a scar from an operation on his neck. He has stuttered "as long as he can remember" and encounters the difficulty in both Salish and English, although more so in the latter language. His speech is most noticeably broken by a disruption of his breathing in the form of gasps before words and extremely long hesitations between words and phrases, with some

3 Other informants stated that there were quite a few stutterers from his reserve.

syllable repetitions. As he blocks on words he blinks and draws his head down towards his chest, all of which is associated with marked spasmodic contractions of the diaphragm. The blocks are experienced at home but more at school, where he has become increasingly sensitive. He says that the other children laugh at him, and it makes him feel bad.

This case in our estimate exemplifies stuttering as it appears in White persons in our own society in several important particulars, namely in the presence of word blocks, anxiety, and awareness of social disapproval. We note especially the so-called "secondary" manifestations of stuttering in the blinking of the eyes, drawing the head down, and the disturbed breathing. These symptoms, which are commonly interpreted as a function of the person's anxiety or anticipation of probable social penalties of the defect, also come out in several other cases. In the one other individual we interviewed they assumed the form of covering the mouth with his hand, breaking eye-contact, and fiddling with his necktie. In another reported case the man "danced up and down to get the words out," and in still another the man "squeezed his eyes shut," often giving up the struggle to speak by announcing that he could not say the word.

Evidence for the aboriginal existence of stuttering among the coastal Indians in the Northwest is somewhat attenuated but still impressive. First of all, the individual we interviewed, who had stuttered as a child, was seventy years old. Allowing sixty years for the period since he stuttered we are removed to 1890, at which time the degree of direct interaction with Whites by the Indians of this man's band was almost negligible. The first priest, for example, did not visit his band until 1880. Prior to that contacts with Whites were largely trade contacts. Even today this band remains quite isolated geographically and socially. Another informant at Nanaimo, who was eighty years of age, spoke of a stutterer who was a grown man when he (the informant) was a boy. Here the first mission and mission school were not established until some time after 1865, so that the childhood of this stutterer dated back to a period when White religious and educational influence must have been at a minimum.[4] Sapir's informants, who described the speech disorders of the Nootka, were young adults at the time of his field work in 1910. This means that the data pertain to the historical period from 1875 to 1890 on, the earlier dates barely coinciding with the establishment of missions and schools on the west coast of Vancouver Island.[5]

A somewhat different type of data pointing to the early, pre-White contact

4 Thomas Crosby, *Among the An-ko-me-ums* (Toronto, 1907), ch. 1.

5 Edward Sapir, *Abnormal Types of Speech in Nootka* (Anthropological Series, Canada Department of Mines, Geological Survey, Memoir 62, no. 5, 1915), p. 3.

occurrence of stuttering among the coastal Indians came from descriptions given us of several native methods of "curing" stuttering. For example, the Nootka had a practise of striking the stutterer on the arm to help him get the words out. According to the Comox, stuttering "could be cured" by having the afflicted person take a bite of food and pass it from one hand to another under one knee before placing it in the mouth. This had to be done four times. The Tlahoose and the Homalthko treated stuttering even more uniquely; the stutterer was instructed to find a board with a knothole in it, and every morning on arising recite to it *ha'ak'ok chen,* "I give my stuttering to you," and then spit or blow through the hole in order to get the stuttering out of his throat. Also in these bands a shaman could be hired to cure stuttering, using a special native medicine in the process.

The likelihood that well defined methods of treatment for stuttering such as we have described arose after culture contact and interaction with White populations seems improbable. We are inclined to believe that a considerable length of time would be necessary for the evolution of such folk techniques, or that if they were non-aboriginal inventions they would reveal signs of White influences.

Perhaps even more telling proof of the very early, pre-acculturative presence of stuttering in the North Pacific Indian areas lies in the widespread linguistic recognition of the disorder. Every tribal language of which we inquired contained words for stuttering as well as for certain other speech disorders. As far as we could tell there were no borrowed English words nor any intertribal borrowings of words to designate stuttering. The various tribal terms for the disorder are as follows:

Salish	
Homalthko, Sliammon, Tlahoose	*ha'ak'ok*
Comox	*hahk'ok*
Nanaimo	*skeykulskwels*
Cowichan, Seschelt, Squamish	*o'tsa'tsa*
Timshian	
Nass River	*ah'al*
Kitimat	*amkwa*
Haida	
Skidegate	*kilekwigu'ung*
Kwakiutl	
Nimpkish, Kingcome Inlet	*kaketsakwa'*

The Nootka of the west coast of Vancouver Island merit special comment with respect not only to the cultural recognition given stuttering, but also for the way in which their language has been modified and adapted to reckon with a relatively wide variety of speech defects. Our two Nootka informants described three and four different speech defects respectively in their accounts, both including stuttering. The older of the two, from Nootka Sound, spoke of the following: *hahuchin,* stuttering; *tlatlachkin,* blocking or "pinching off" words; *ninichin,* straining to get words out. The other man, from the reserve at Port Alberni, gave a somewhat different set of four categories: *hachucha,* stuttering; *hahuting,* lisping; *tlatlaching,* "collapsed speech"; *ninichin,* talking through the nose (probably cleft palate speech).[6]

In Sapir's earlier research on the Nootka he described, in all, five speech abnormalities, although two of them probably can be reduced to variants of lisping. The other three are nasalization, stuttering, and cleft palate speech. He gives no native term[7] for stuttering, which frees us to believe that the term we obtained (*hahuchin*) may well be the proper one or its close approximation.

ATTITUDES TOWARDS STUTTERERS

The attitudes of the coastal Indians in the North Pacific towards stutterers range from mild to serious disapproval, through humorous tolerance, to pity and condescension. Members of the three mainland tribes, the Homalthko, Sliammon, and Tlahoose were easily moved to laughter by mention or discussion of the subject. Their reaction is best summed up by the statement of one man who said: "Oh, we laugh at such people at first but then we get used to them." Another man interviewed on the Nanaimo reserve expressed essentially the same attitude, adding the observation that "outsiders" continued to laugh at stutterers even though their immediate associates came in time to accept them without comment. Several of the undirected, freely associated stories which we had children of the Port Alberni residential school write on the subject of "A Person Who Couldn't Talk Right or Who Stuttered" conveyed ideas that in the Bella Bella and Tsimshian areas parents tended to be shamed by their children if they stuttered. This is not unexpected in the light of the high standards of

6 In transcribing the native words here we have employed the method suggested by Sapir and Swadesh for using native terms in English context (Edward Sapir and Morris Swadesh, *Nootka Texts,* Philadelphia: Linguistic Society of America, 1939, p. 13).

7 We have not attempted to reproduce Sapir's phonetic transcriptions of the native terms used in this older research. Later he adopted a different system. Readers who are interested may consult his *Abnormal Types of Speech in Nootka,* p. 20 f.

decorum for children, especially of high ranking parents, held in the old Tsimshian culture, in which, among other stringencies, children were forbidden to run in their play lest they accidentally fall and thus shame their parents.[8]

A more specific demonstration of negative attitudes towards stutterers came from our case history of a Tsimshian stutterer. His father severely criticized him as a child for his faulty speech, invoking the native term for "acting crazy" (in contradistinction to insanity) to express his strong disapproval of his son's verbal failures. This concept was generally applied to anyone deviating in disapproved ways from the norms of the culture. This man's father also had told him disapprovingly that in former days boys with speech defects had to take a back seat in the potlatch ceremonies and could not actively participate in them. A Haida woman confirmed that this was also the case in her tribe in former times. A Kwakiutl spokesman said he had known of stutterers but had never known of one who took part in a potlatch. However, all of this does not mean that anyone in these tribes was free to insult a person because of his speech difficulties, for there was always the threat that the family of the offended man or woman would give a potlatch to shame the person or clan responsible for the insult.

It is from the Nootka that we get the most abundant evidence of social penalization of persons with physical and speech defects. In this culture fat persons, very small persons, sore-eyed, squinting, cock-eyed, hunchbacked, lame, circumcised, left-handed people and those with different speech disorders became the object of pity, mockery, satirization, humor, and patronage of the sort shown small children. This is most apparent in the myths of the Nootka, but emerges with equal clarity in associations of animals, mythological characters, and defective persons with various linguistic and stylistic processes involving the symbolic use of sounds. Thus when a person who had a speech defect was addressed or talked about, his defect was imitated or mocked, with changes in word forms—such as suffix changes—which carried implications of smallness, childishness, animal quality, or of some physical defect.[9]

The analysis of myths occurring throughout the broader culture area of the Northwest reveals much the same social rejection and social isolation of such child deviants as orphans, crippled, blind, stupid, dirty, lazy, weak, and those with scabs or skin diseases. Most of these myths are analogues of the success stories of our own culture and have been specifically analyzed as variants of the

8 Viola Garfield, *Tsimshian Clan and Society* (University of Washington Publications in Anthropology, vol. 7, 1939), p. 193; if our information is correct, this also held for the old Nootka culture. The Kwakiutl likewise were shamed by injuries to their children (see Helen Codere, *Fighting with Property*, Monographs, American Ethnological Society, vol. 18, 1950, p. 80).

9 Sapir, *Abnormal Types of Speech in Nootka*, pp. 3-15.

Cinderella theme in Grimms' fairly tales.[10] It is perhaps significant that the Swaixwe mask, whose mythological origin concerns a boy who gets cured of a bad skin disease and is enabled to invent and use a new dance ceremony, has the widest distribution of any mask in the Northwest.[11] While stutterers are not mentioned in any of these myths, we found in several of the stories written for us by the residential school children there were equivalents of Horatio Alger-like stutterers who overcome their defect to go on and become famous. Since the analysis of the myths was a content analysis and not a stylistic or linguistic one, we do not know whether abnormal speech forms were used in telling the myths as was found by Sapir in Nootka.

With the Nootka these abnormal speech forms could have varying meanings of mere smallness, contempt, or even pitying affection depending upon the nature of the relationship between the speaker and the person addressed or discussed. Imitation of the defects of one person might be done in a friendly spirit, whereas the same mimicry of another would be an insult. Furthermore, it seems that some restraint was exercised in mocking adults when they were present, although this was not the case for children. An expression of this qualified attitude towards persons with speech disorders is seen in the application of special names by the Nootka. Our informant recalled, for example, one lame man who was known as "rotten foot," another known as "the man whose wound healed with skunk cabbage in it," and lastly, of more direct interest, one called the "stuttering man." He said that such names could be given the person either by himself or by close relatives. However, it would be insulting for unrelated persons to impose the name, and only such a man's close friends were free to address him in this fashion.

Insofar as our data permit us to say, the Nootka differed from the Tsimshian in respect to the more formal discrimination against persons with speech disorders. Being a stutterer did not bar one from giving a potlatch if he were otherwise able and entitled to do so. A stutterer who formerly lived at Barclay Sound was the head man of one of the four clans there and took part in and gave numerous potlatches. It might be added that this man was exceptionally well-liked for his personal characteristics. It should be borne in mind that while a high-ranking Nootka might make his own speeches at potlatch ceremonies, still it was culturally permissible to hire professional speakers for this purpose. However, doing this to show a person's wealth would be quite different from doing it because the person was an ineffective speaker.

10 Betty Randall, "The Cinderella Theme in Northwest Coast Folklore" (in *Indians of the Urban Northwest*, Marian Smith, ed., pp. 243-285, New York, 1949).

11 Helen Codere, *The Swaixwe Myth of the Middle Fraser River . . .* (Journal of American Folklore, vol. 61, no. 239, pp. 1-18, 1948).

THE RELATION OF STUTTERING TO CULTURAL FACTORS

With the time and resources available our research could not result in the collection of data which would permit testing of any considerable number of the current theories of stuttering. Some isolated facts may be taken as bearing upon one or two of these theories. For example, there seems to have been very little mental disorder among the Nootka,[12] which may be interpreted to mean that speech disorders there were unlikely to have been the function of neurotic or personality disorders. The theory that stuttering is related to dysphemia and changed-handedness gets some oblique support from the recorded aversion of the Nootka to left-handedness and their rather painful method used to discourage children from using their left hands.[13] However, the limited nature of our knowledge of these things can permit little beyond speculation.

The particular data we secured and those available in ethnographies of the Northwest coastal culture areas lend themselves far more readily to testing sociopsychological theories of stuttering which make room for the interplay of cultural factors. Here we have in mind the explicit recognition of the importance of such factors in Johnson's "evaluational" theory of stuttering and their implicit emphasis in Van Riper's formulation of the growth of stuttering as a process.[14] Both theories, which have been widely influential in the study and treatment of stuttering, lay stress upon (1) the penalizing reactions of others, especially those of anxious parents, critical siblings, and ridiculing playmates, towards the speech of the young child; (2) the internalization of these reactions or negative evaluations by the child; (3) the growth of anxiety in the child about his own speech; and (4) the pathological disruption of the child's speech rhythms by this anxiety.

The main difference in the two views regarding the development of stuttering concerns the appearance of the initial symptoms of stuttering, with Van Riper holding that disorganization of the child's immediate social environment can produce generalized conflict or anxiety, of which the stuttering is an expression.

12 Robert Brown, ed., *The Adventures of John Jewett* (London, 1896), p. 175.

13 Phillip Drucker, *The Northern and Central Nootkan Tribes* (Bulletin, Bureau of American Ethnology, no. 144, 1951), p. 130. We found one Homalthko woman whose parents had attempted to change her from left- to right-handed, but there was no stuttering in her family.

14 Wendell Johnson, *People in Quandaries*, ch. 17; also his *The Role of Evaluation in Stuttering* (Journal of Speech Disorders, vol. 3, pp. 85-89, 1938); Charles Van Riper, *The Growth of the Stuttering Spasm* (Quarterly Journal of Speech, vol. 23, pp. 70-73, 1937); also his *Speech Correction* (New York, 1947), pp. 280-290; Charles Van Riper and Wendell Johnson, *Stuttering* (Chicago: National Society for Crippled Children and Adults, 1948), pp. 24-33.

At this stage of the "primary symptoms," the reactions of others enter by the above indicated process to create the exaggerated blocks we commonly recognize as stuttering. Johnson apparently believes that early as well as late stuttering is entirely due to the way in which others *define* (evaluate) the normally variable speech of children.

Our data can best be cited to test the more general sociopsychological proposition upon which there seems to be agreement in the theories of the two writers to whom we have referred. Insofar as it is possible we will examine data which bear upon the second, perhaps less important, point on which there is divergence of viewpoints. The most significant sociocultural factors, of course, are those which affect parent-child interaction, for there seems to be a consensus that practically all stuttering begins in childhood. Consequently a consideration of child-rearing practices in relation to stuttering among the Northwest coastal Indians is directly relevant.

No trait of primitive society, particularly in North America, has been more widely commented upon than that of parental indulgence of children.[15] The primitive cultures of the North Pacific prove to be no exception with respect to this characteristic. In general the reconstructed picture of primitive practise in this area is one in which corporal punishment plays a minor part or is absent entirely in the discipline of children. One exception we discovered, in recounts of the old culture of the mainland Homalthko, Tlahoose, and Sliammon, serves to emphasize the generalization. Here a child, if extremely recalcitrant, might be spanked with a beaver tail in front of other children of the band. However, this was always done by two persons especially designated for the job, never by the parents. The socialization of the child generally seems to have been brought about through teaching by example, long moral lectures and morality stories—often repeated—given by the grandparents, and private scoldings heavily laced with the shame motive. Threats seldom if ever entered into these.[16]

Granting what seem to be obvious similarities between the child rearing methods of the northern Pacific Indian cultures and those in the rest of North America, it does not follow that the standards of childhood decorum, aspiration, and expectations of the child's ultimate participation in the formal rituals of the culture which served as goals of the educational process were comparable. Indeed it is precisely here, in the rich ceremonial solo drama and theatricality of feasts and potlatches captioning the life crises of the individual, that we perceive the

15 George Pettitt, *Primitive Education in North America* (University of California Publications in Archaeology and Ethnology, vol. 43, 1946), p. 6.

16 Drucker, *The Northern and Central Nootkan Tribes*, p. 131.

differentiation of the Northwest coastal Indian cultures from those broadly conceived as North American. The importance of the ceremonial vindication of the individual's position in society was impressed quite early with the indoctrination of adult values and goals, beginning almost at birth. Parents in the Nootka tribes often began their discourses on mores while the infant was still cradle-bound, with high ranking fathers seriously lecturing sons at this time on the merits of giving feasts and potlatches. Most Nootka children while still quite young were expected to learn their privileged dances and sacred songs, and adults worked continuously with them to achieve the necessary competence in their performances. At very tender ages they were required to perform alone before assemblies of their own and other tribesmen. The Nootka ordinarily initiated children into the Shaman's dance at the age of seven or eight years; a chief might have his one-year old son inducted as a novice, or even have his pregnant wife treated as a "novice" as a means of initiating his unborn child into the adult ceremonial life.[17]

This ritual, more or less central to the whole ceremonial complex of the Nootka, by its rough nature was undoubtedly a traumatic experience to many of the small children who were necessarily spectators and participants. On this point Drucker says:

> Small children were usually frightened during the performance . . . the hullabaloo and running about, the rough practical jokes (carried to the point of throwing people bodily into the sea), kidnapping and purported slaying of siblings and playmates all combined to make small children the least entertained and most perturbed of the spectators.[18]

17 Drucker, *op. cit.*, pp. 386 ff; the same early initiation of children into special societies occurred among the Tsimshian, see Garfield, *Tsimshian Clan and Society*, p. 299. The desire to make the child into a "little adult" appears to some extent in Salish culture even today (see Joanne Schriver and Eleanor Leacock, "Harrison Indian Childhood" (in *Indians of the Urban Northwest*, p. 199). Apparently from early times the Kwakiutl expected adult type behavior in children (C. S. Ford, *Smoke from their Fires*, New Haven, 1941, p. 56, also all of ch. 3).

18 Drucker, *op. cit.*, p. 130. There are a number of other characteristics of the Northwestern Pacific primitive cultures which might be thought of as anxiety-inducing for the child. The large households apparently continually underwent change with comings and goings of relatives and others. Marriage was generally associated with considerable conflict of the spouses and divorce was fairly common, followed by shifts in residence of mother and child. Warfare presented the constant threat of death or enslavement of children. The ideal warrior was one who was utterly bloodthirsty and terrible in aspect. Several of our Salish informants recalled the great dread they held as children towards warriors of their own tribes, a dread which remained very real to them as adults today. Whether such cultural phenomena gave rise to abnormal anxieties or whether cultural techniques were adequate to permit the child to handle such anxieties is a large subject which we cannot undertake to discuss here.

By the time children were old enough to become active participants in the Shaman's dance it is true that their fears inspired by it tended to disappear, but in their place came anxieties as to whether they would "remember their instructions" and whether they would perform adequately.[19] This anxiety, when it occurred, clearly seems to have been a reciprocal of socially invidious distinctions and the praise-rewards reactions of adults in terms of competency in performing rituals. In this connection it is informative to read of an Ehetisaht Shaman's dance in which the "wolves" going through the village announced that they wanted only those novices who could sing their songs and sing them well.[20] An observer as far back as 1868 tells how Nootka boys in their evening play on the beach frequently mimicked famous orators of the tribes and recited parts of legendary speeches before an audience of old men who pointed out this boy or that boy as destined to do great things.[21] That parents of the Nootkan tribes had anxieties concerning the future verbal facility of their children seems demonstrated by the custom of putting a finger far down in the infant's throat to insure his becoming a good singer.[22]

It is to be noted that Nootka culture admitted of little or no deviation from ordinary rules of inheritance of songs or rituals, private or public, simply because the proper heir could not learn them. But along with this a person did not automatically gain acceptance in his rank purely by inheritance; it was necessary to show his right to social position by his personal traits, proper conduct, and ritualistic versatility. From the standpoint of the child the standards of the culture were not only rigorous and exacting but also inexorable; and their early imposition indubitably played a significant part in creating a very shame-conscious person, highly sensitive to his own behavior deviations and to those of other persons, including members of his own family. That this sensitivity extended to speech behavior is emphasized by Sproat's early remarks on the "standard of

19 *Idem*, p. 138. A Kwakiutl man to whom we talked said that his going through the *hamatsa* ritual when he was twelve years old caused him much anxiety and that his grandfather practically forced him to do it. A Comox man told of similar feelings at his first potlatch. Potlatching among the Kwakiutl called for careful memory of numerous ancestors' names, their histories and social positions, as well as the social positions of all living ranking tribe members. In addition entry into potlatching meant one could not be "foolish" in spending his money. It is not unusual that many children were more or less compelled even to enter into "play potlatches" (see Ford, *Smoke from Their Fires*, chs. 3, 5, p. 86).

20 Drucker, *op. cit.*, p. 149.

21 Gilbert Sproat, *Scenes and Studies of Savage Life* (London, 1868), p. 63. The Kwakiutl did not attend "play" potlatches of their children but nevertheless kept informed about them and criticized their conduct later (Ford, *op. cit.*, p. 85f).

22 This was also related to the desire to make the child physically attractive (Drucker, *op. cit.*, p. 123).

correct speech" held by the Nootka and the extreme care with which they pronounced names and words they heard for the first time.[23]

The competitive, rank-conscious stress of the culture undoubtedly made the deviant person especially vulnerable to mimicry, humorous anecdotes, and gossip, and, although the cultural ideal specified that the deviant accept all of this in good grace, it seems unlikely that he was able to do so. Thus, in speaking of persons who are *o'ucshaid* (bunglers, clumsy ones), the Nootka say they are teased without malice and that they tell stories on themselves, but also insightfully add that the teasing "seems to make them worse; they do more and more funny things."[24] In the case of the stutterers it seems clear that they would be at the additional disadvantage of not being able to tell stories on themselves or to effectively use verbal aggressions when goaded beyond endurance.

While the general adult population of these Northwestern Indian tribes may have been partly restrained in criticizing the stuttering child, in all likelihood this was not true for his immediate family. It is also probable that in play groups the stutterer aboriginally encountered much open ridicule from other children—especially away from the presence of adults. The only deterrent here seems to have been cautions from parents to their children to be careful what they said to sons of chiefs. We guess that such cautions were ignored as frequently by, say, Nootka children in early times as comparable injunctions are by children in our society today.

The potlatch, of course, was the main defense of the deviant against insult, but it is questionable whether the speech deviant was free to avail himself of the device, for by its very nature it would lead to a public display of the behavior occasioning the insult. Even if relatives or a friendly chief gave the potlatch for the insulted person, his defect would still be publicized. Indeed potlatches of this order seem to have been used more for wiping out shame related to incidental deviations from norms and accepted practises rather than that associated with a permanent or continuing social inadequacy.

CONCLUSION

There is fair to good evidence that stuttering is present among the people of the northwest coastal Indian tribes and for the probabilities of its aboriginal incidence. The cultural setting in these Indian societies in the past as well as in

23 Sproat, *op. cit.*, p. 266. In aboriginal times death was sometimes the penalty for mentioning tabooed names or words associated with them (Drucker, *op. cit.*, p. 149).

24 Drucker, *op. cit.*, p. 328.

the present seems favorable to the development of stuttering. The facts indicate that aboriginally stuttering was culturally defined, that stutterers were and still are socially penalized, that parents tended to be specifically concerned or anxious about the speech development of children, that children were anxious about ritual performances involving solo verbal behavior, and that Indian stutterers showed and still show "secondary" symptoms of the defect, which are ordinarily interpreted as distortions of the speech induced by internalizations of anxiety about the speech.

We have no particular reason for questioning the authenticity of Johnson's and Snidecor's reports of an absence of stuttering and concomitant cultural definitions of the defect among the Bannock and Shoshone, inasmuch as their sample was large and as specialists in their field they presumably knew what they were looking for.[25] Consequently we are left with the possibility that a real differentiation exists between these cultures and Northwest Coast Indian cultures in these respects. However, such differences are not incompatible with the general nature of the theory which we have been interested in testing. Indeed one study of child development among the Wind River Shoshone gives much evidence that the culture differs sharply from the cultures we have described with respect both to degree of pressure on children and treatment of defective persons.[26]

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25 *Supra.*

26 D. B. Shimkin, *Childhood and Development among the Wind River Shoshone* (Anthropological Records, University of California, vol. 5, no. 5, 1947), pp. 289-325.

SOME BASIC METHODOLOGICAL PROBLEMS IN PREHISTORY

J. S. SLOTKIN

IT IS USUALLY ACKNOWLEDGED that all historical interpretations are made in terms of implicit or explicit generalizations about relevant processes. Also, that these generalizations are derived primarily from a study of contemporary phenomena. Thus, historical geology is based upon a study of modern geological processes; paleontological history, upon a study of modern genetic and biological processes; and human history, upon a study of modern social, social psychological, or psychological processes.

My thesis is that there are certain methodological problems involved in these procedures, and that the problems are particularly serious in the field of human prehistory. They arise from the fact that the processes used in the interpretation of prehistory are customarily based upon generalizations derived from a study of contemporary man.

It is logically valid to infer that generalizations derived from a sample of an homogeneous class (in this case, from contemporary *Homo sapiens*) are applicable to other samples of that class (e.g., prehistoric groups of *Homo sapiens*). It is logically invalid to infer that these generalizations are applicable to samples from other classes (e.g., prehistoric groups of Neanderthal, Sinanthropus, etc.). To make the latter inference is to be guilty of the logical fallacy of the undistributed middle. It is also empirically invalid to make such an inference, for it assumes that there are no significant differences in the behavior of organisms from different species and genera, which is probably false. Therefore it may be correct to use generalizations derived from modern man in interpreting the prehistory of *Homo sapiens,* but it is incorrect to use those generalizations in interpreting the prehistory of other hominid forms.

I can conceive of only one solution to this difficulty, and that is by a comparative study of the relevant processes in all, or a representative sample of, the higher primates, i.e., monkeys, apes, and man. Whatever generalizations hold true for all these primates have a high degree of probability of also having been common to the lineal ancestors of these primates. For, by the addition of probabilities, there is a very low degree of probability that each of the primate forms independently developed these common processes. This, in effect, was E. A. Westermarck's solution in *The History of Human Marriage.*

But there are certain difficulties to this solution.

In the first place, its degree of probability is vitiated by the fact that the sample from which it is derived seems to be neither random nor representative. In regard to randomness, the theory of evolution states that contemporary species are the result of a process of natural selection. Therefore the less adaptable forms have become extinct and the more adaptable have survived. Thus the contemporary primates constitute a naturally selected sample of primates, and not a random sample of primates. It may also be that the contemporary primates are not representative of all primate forms of behavior. This is particularly probable for the hominids, since man adjusts primarily by means of socially acquired, rather than inherited, forms of behavior, as A. R. Wallace pointed out many years ago (*Natural Selection and Tropical Nature, etc.*). Hence there may have been other primate forms in the past who behaved in different ways and became extinct because such modes of behavior were non-adaptive. Consequently a comparative study of modern primates may reveal processes which were common to their lineal ancestors, but these processes need not have been common to their collateral relatives.

In the second place, even if the processes discovered by a comparative study of contemporary primates are applicable to all extinct hominid forms, the question arises as to the significance of such interpretations. How effective would an interpretation of the history of contemporary human society and culture be, if restricted exclusively to the processes common to all the higher primates? Note that this would exclude any but the rudimentary forms of symbols and tools, traits which are fundamental in the society and culture of *Homo sapiens*.

To sum up, then, the only approach to prehistory which seems to have any logical or empirical validity, when applied to extinct hominids, is that based upon comparative primatology. Even so, there are difficulties. But there is certainly neither logical nor empirical validity to the customary procedure of interpreting the prehistory of extinct hominid forms in terms of processes derived exclusively from a study of modern man.

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INTERGLACIAL ARTIFACTS FROM THE SAN DIEGO AREA

GEORGE F. CARTER

INTRODUCTION

ARCHAEOLOGICAL WORK in southern California beginning in 1930 has led to ever widening studies aimed at gaining a better time perspective concerning the age of man in America. In the summers of 1947, 1948, and 1950 studies were made of the relationships between soils and terraces in hope of establishing a time scale from soil-ageing phenomena that would be applicable to archaeology.[1] This led to examination of both ocean front (sea) terraces and river terraces, and the working out of geological sequences. A paper on one Pleistocene landform containing artifacts has previously been published.[2] In 1947, artifacts were unexpectedly found in a terrace that is probably of third interglacial age. More were found in 1950. These are illustrated and the circumstances reported here.

PEDOLOGIC BACKGROUND

Recognition of the age of alluvial formations can be approached from several points. The two principal means employed here are pedologic and geomorphic. Certain assumptions underlie the pedologic approach. It is assumed that we can distinguish between soils produced under arid, semi-arid, and humid conditions. Regional (as opposed to local) accumulation of carbonates, especially calcium carbonates, at a relatively shallow depth is considered a mark of semi-arid climates where the moisture relationships are such that there is little or no percolation of rainwater down to a permanent water table. In humid regions such through-going percolation results in the removal of the more soluble minerals, of which calcium is one of the most conspicuous. Mature soils in moist regions, therefore, are lime-poor or lime-free and tend to be acid in reaction. Soils of semi-arid regions on the contrary tend to accumulate lime. The depth of accumulation is related to many factors such as parent material and slope, but in a regional sense it is determined by amount and distribution of rainfall. "Soils and Men" gives a figure of about twenty-five inches of rainfall as being

1 These studies were supported in 1947 and 1948 by grants from the Isaiah Bowman School of Geography, and in 1950 by the Axel Wenner-Gren Foundation.

2 George F. Carter, *Evidence for Pleistocene Man in Southern California* (Geographic Review, vol. 40, pp. 84-102, 1950).

the approximate boundary in the Temperate Zone between lime-accumulating and lime-impoverished soils.[3]

The significance of these observations for soils in the San Diego region are clear. This region now has an annual average rainfall of less than 10 inches. It is clearly on a steppe-desert boundary and should have lime-accumulating soils with the zone of lime accumulation at very moderate depths. A pre-pottery site in Soledad valley (W-316) shows a carbonate accumulation at 12 to 18 inches which in the opinion of Dr V. P. Sokoloff reflects the current pedogenesis. Jenny's table[4] would suggest a lime pan to be expectable at about 10 inches. Actually the bulk of the mature soils of the San Diego coastal area are characterized by clay pans and acid conditions with lime, if present, accumulating at depths of three to five feet or more. These conditions suggest a climate with twenty to thirty inches of rainfall. Such soils are, therefore, considered to be a heritage from a pluvial period, the local equivalent of glacial conditions. Soils accumulating lime at shallow depths tend to be limited to middens where refuse material of Recent age has weathered sufficiently to allow a distribution of soluble materials normal to the present aridity. Soils then provide a clear division between humid-pluvial-glacial times and Recent-arid times.

GEOMORPHIC BACKGROUND

The sea level during Pleistocene has fluctuated between considerable elevations above to considerable elevations below present sea level. It is estimated that sea levels fell in the neighborhood of three hundred feet below present levels during the last glacial period. It is clear that sea levels were higher than the present during interglacial times when the ice caps were wholly or partially melted off. There are worldwide evidences of these past high and low sea stands. Their effects dominate the coastal regions of most of the world. In Europe high sea-stands can be correlated with interglacial fauna and with Paleolithic cultures. In America similar evidences suggest that our terraces correlate with those of the rest of the world.[5]

The Pleistocene terraces of the San Diego Bay region have been studied and named by Ellis and Lee.[6] Field work shows that these terraces can be more sharply

3 "Soils and Men" (*Yearbook of Agriculture*, 1938), p. 970.

4 Hans Jenny, *Factors of Soil Formation* (1941), fig. 60, p. 122.

5 C. W. Cooke, *Pleistocene Seashores* (Journal, Washington Academy of Sciences, vol. 20, 1930, pp. 389-395), and various later papers; also Ernst Antevs, and others.

6 A. J. Ellis and C. H. Lee, *Geology and Ground Waters of the Western Part of San Diego County, California* (United States Geological Survey, Water Supply Paper, no. 446, 1917).

delimited than these authors did in their pioneering work, and that they can be traced far beyond the San Diego Bay area to the north and south and correlated with the wave-cut cliffs of the ocean front. The few students of the geology of this area all have commented on its apparent stability during at least later Pleistocene time.[7] An interpretation of the sequences of terracing, based on the work of others as well as field study, is given below.

In the San Diego region the extensive Pleistocene terraces begin below the 300 foot level. There is a distinct series of terraces and deposits at about 250 feet that are clearly earlier than the lower-lying comparable materials. This is evidenced in their truncation by later terraces, by their greater dissection, and by their more ancient and complex soils. These are considered to represent the first interglacial period.

The most striking series of terraces and deposits are found about the 100 foot level. The river valleys were filled with sands and gravels, and a very broad marine terrace was cut into Mt Soledad, nearly removing the earlier Pleistocene terrace surfaces. It is considered that, as elsewhere in the world, this 100 foot sea-stand of long duration represents the great interglacial of mid-Pleistocene time.

With the onset of the third glacial period the sea levels fell to some unknown level, and the rivers cut out their valley fills, leaving remnants as valley terraces along their flanks. The third interglacial led to renewed valley filling, but again to lesser heights than the preceding interglacial. The last glaciation led again to valley excavation. Well logs show that the valleys were deepened to far below present sea levels. Finally, Recent time has been marked by rising sea levels and alluviating valleys.

It will be noted that all of the valley fills are equated with interglacials. The vexing question of a 15 or 25 foot postglacial high sea-stand must be dealt with here, even if very briefly. I have no evidence of such a sea-stand either in the San Diego Bay region or the Chesapeake Bay region. In the San Diego area mature soils characterized by strongly developed clay pans extend to tide level today. In the south San Diego Bay region such clay pans form the resistant layer at the water's edge in the vicinity of the Rohr Aircraft plant. If there had been a 15 or 25 foot sea-stand within postglacial time, one would have to assume in still later time climatic conditions appropriate to the formation of such soils and of sufficient duration to weather these materials to maturity. Our findings suggest that a mature clay pan soil may require a minimum of 10,000

7 See Carter, *op. cit.*, 1950, p. 92, for a brief review of these statements.

years of humid climate for its formation.[8] The clay pan soils of the San Diego region are lime-poor, reflecting a long period of thorough leaching which is inconsistent with the present climate, and extend down close to modern sea levels, and are associated with Wisconsin age alluvial features. The usual estimates of 6 to 8,000 years ago for such a high sea-stand accompanying the postglacial warm period do not allow sufficient time for such climatic changes and soil development. The tendency to halve these figures by following the C-14 dating method only makes the case for a postglacial high sea-stand of any magnitude the more improbable.

The valley fills are therefore considered to be of interglacial age, with those about 200-250 feet being of probable first interglacial time, those about 100 feet being deposited in mid-Pleistocene time, and those about 25 and 65 feet being of third interglacial age. If there are any postglacial high sea-stand deposits, they lie at very low levels, probably not above 5 or 6 feet above present sea levels. Even these may not be of Recent age but may refer to some episode in Wisconsin time. This evidence is paralleled in Maryland where these terrace heights are repeated and where mature soils of greater than Wisconsin age approach modern sea levels. Had sea levels of Recent time reached an elevation even of 15 feet above modern levels these Maryland soils either would have been destroyed or recognizably altered by salinization.

It is concluded, therefore, that the valley fills are interglacial in age except for the material in the modern valley floor, which is of Recent age. Artifacts within the fills above the modern floor, unless intrusive, must then be of the age of the fills. It must be noted that while a 100 foot valley fill could readily be truncated and thus come to appear to be a remnant of some lower and later valley filling, it is virtually impossible to make a 25 foot valley filling reach the elevation of a 100 foot valley fill. These interpretations should err then on the conservative side. In most cases, however, the sequence of terraces makes it quite evident which terrace one is dealing with.

TERRACES AT THE OLD MISSION

The San Diego Mission sits on a prominent terrace on the north side of the valley about ten miles inland from Mission Bay (Figure 1). The present river bed at the Mission is fifty feet above sea level. In recent years the terrace im-

8 G. F. Carter and V. P. Sokoloff, *A Study of the Soils and Land Forms of the Chesapeake Bay Margins* (The Isaiah Bowman School of Geography, The Johns Hopkins University, 1952).

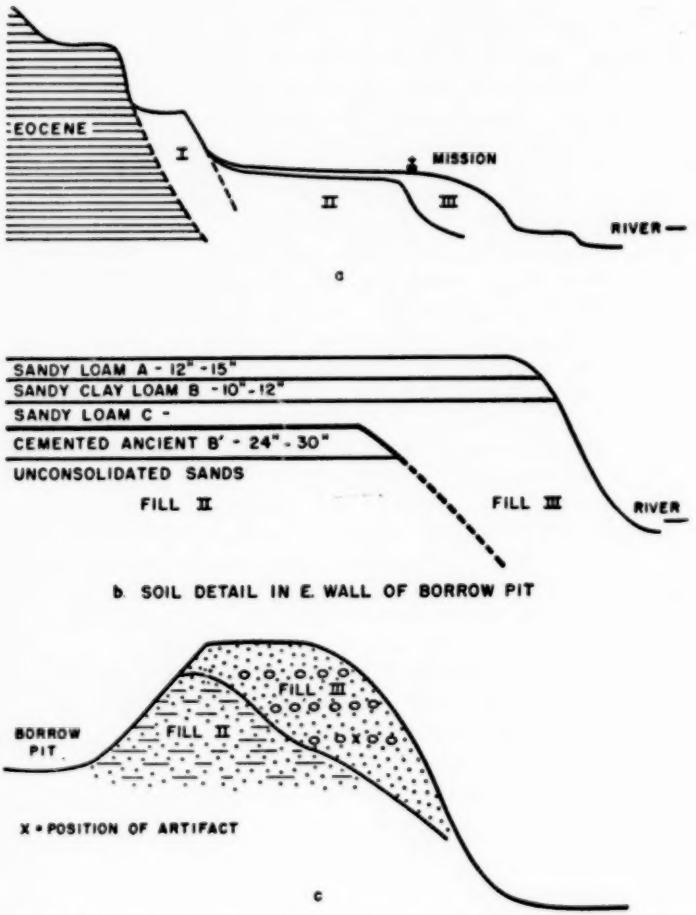


FIG. 1. Schematic cross-sections at the old Mission site, at the entrance to the borrow pit.

mediately west of the Mission has been used as a borrow pit and a large area has been excavated to a depth up to twenty feet. This has exposed superimposed soil profiles (polymorphic soils) that are not in accord with the present climate and can be designated paleosols. The soil horizons mark separate weathering surfaces and reveal the compound nature of this terrace.

The terrace is composed primarily of sands. The valley wall at this point is composed of Eocene rocks in which conglomerates predominate. This material differs greatly from the materials in the terrace. The terrace is, therefore, clearly a valley fill remnant.

Terrace I represents a period of valley filling to more than 200 feet. Terrace II represents a time when the valley was filled to a height of over 100 feet. That this valley fill belongs to mid-Pleistocene time is indicated not only by its appropriate elevation, but by its relative position in the terrace series at this point. This valley fill was cut out, leaving a remnant whose surface and valley face were deeply weathered. The terrace surface developed a soil profile now represented by a cemented horizon up to thirty inches thick, B′ in Figure 1 b. The thickness of this horizon and its degree of cementation represents a period of long duration. The absence of lime accumulation indicates that the weathering was done under humid conditions.

Renewed valley filling laid a new series of sands and gravels against the earlier terrace and in part lapped up onto the early terrace. Subsequently this valley fill was cut out, leaving a remnant plastered onto the older fill. The surface of these fresh materials was then weathered under humid conditions to create a lime-free, clay pan type of soil. Today the river valley is again being filled with alluvium.

This sequence may be reconstructed from the present, a known point in time. Sea levels during Recent time have been rising. This has caused the alluviation of the lower reaches of such river valleys as those in southern California. The preceding period of valley cutting was due to the lowered sea levels of Wisconsin glacial times. The accompanying pluvial climates created the conditions necessary for the development of the lime-free clay pan soil on the terrace. The deposition of the materials in the younger terrace must then date to the preceding high sea-stand, which must have occurred in third interglacial time. The older materials must date to an earlier high sea-stand and thus be referable to the second interglacial period.

In a gravel lense in the younger sands, of third interglacial age, the artifact illustrated in Plate 5 h was found. It was firmly embedded in the sands and gravels beneath undisturbed strata (see Figure 1 c). It is a chopper-like imple-

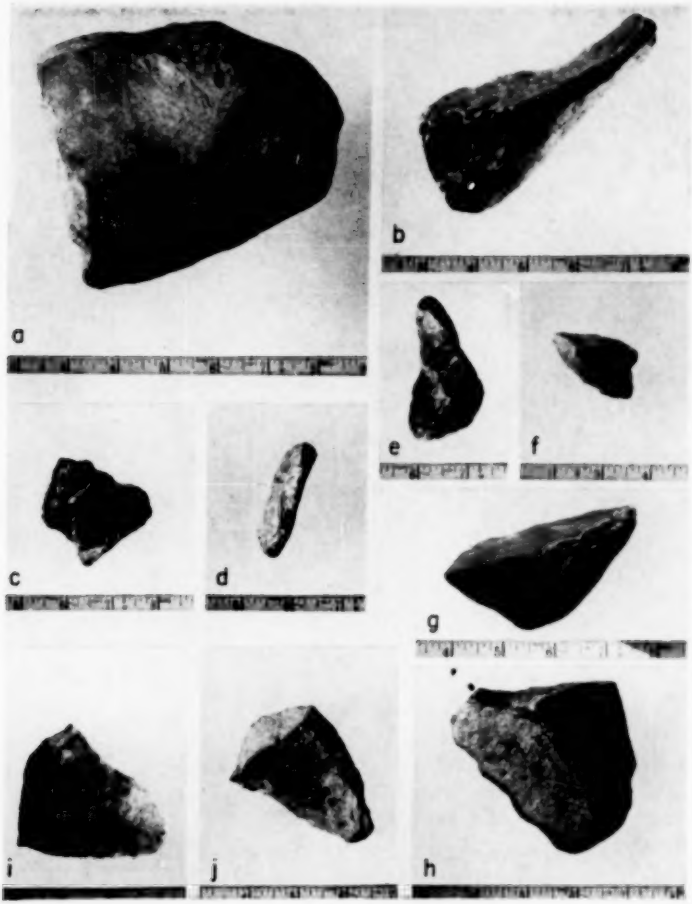
ment made from a quartzite boulder and was heavily worn by stream action before its final deposition. The stone is iron-stained, as are all the stones in this gravel. The iron staining and iron cementation of these sands is further evidence of former conditions much moister than now, for the translocation of the iron family by percolating waters is a mark of a humid climate with the accompanying dominance of percolating waters. The artifact is, therefore, of the age of the enclosing geologic materials. The terrace materials to which this artifact belongs lie today at 100 feet. This does not mean this terrace is of mid-Pleistocene age. The river is today alluviating its valley, and at modern sea levels is here at an elevation of 50 feet. If the sea level stood approximately 25 feet higher, one might expect the river to fill its valley correspondingly, and hence expect alluviation to a level at about 75 feet. That this terrace is higher than that, even lapping onto a terrace of probable mid-Pleistocene time, suggests that the younger materials here, which contained the artifact, are possibly to be attributed to an earlier and higher than 25 foot interglacial sea-stand. They must be accorded a third interglacial age, and possibly an early third interglacial age. In terms of Ellis and Lee's nomeclature, the younger terrace is probably best correlated with the Nestor terrace.

THE SWEETWATER RIVER VALLEY

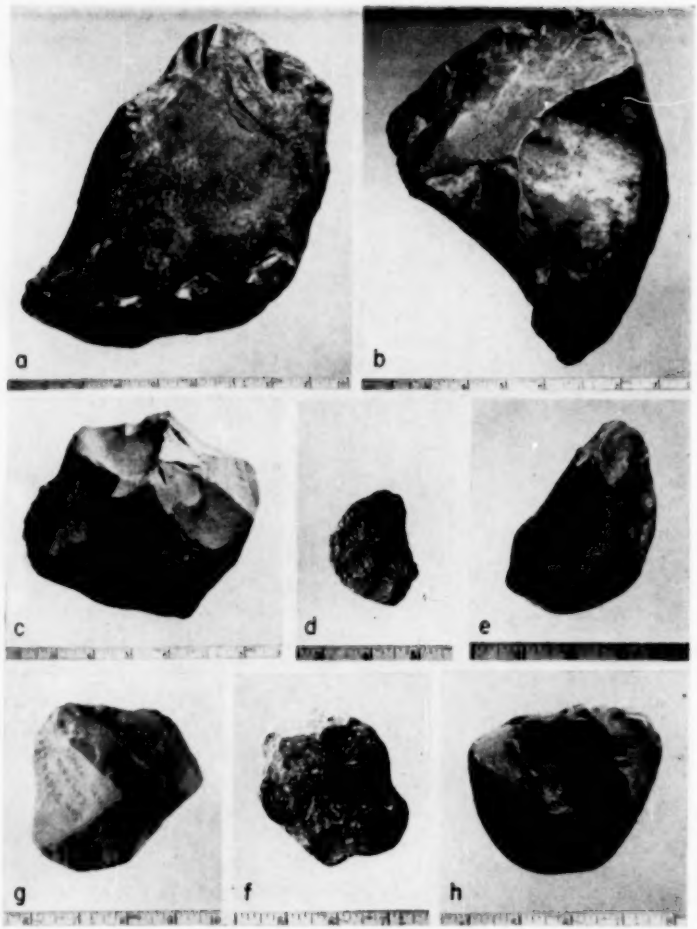
This river valley, nine miles south of the San Diego River valley, has in its lower course a set of valley fills identical to those in the San Diego River valley. This identity extends even to compound valley fills at the same levels as those at the San Diego Mission. This compounding of terraces is most clearly expressed in the valley fill on the north side of the river, one mile south of Bonita.

Stones suspiciously like artifacts were found in a gravel pit just north of National Avenue in 1947 (see Plate 4 e, f). They seemed too questionable for certain identification as artifacts as well as "improbable" in such a position. The site was revisited in 1950 when the artifacts illustrated in Plate 4 b, c-d, g-h were collected. (Linked letters refer to two views of one artifact.) By that date the original borrow pit, which began as a cut into the terraced river bank, had been extended back more than two hundred feet, and the road from Highland Avenue had been extended west to National Avenue (see Figure 2 a).

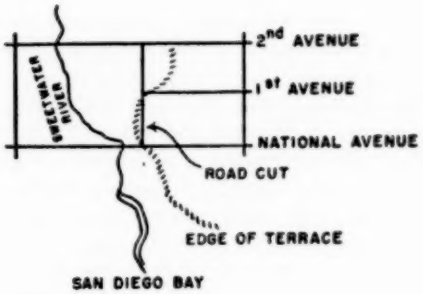
This terrace is capped by a pre-pottery site belonging to the La Jolla culture. It has a very considerable antiquity. Either it was established on the terrace surface after the development of a soil which, developing under humid condi-



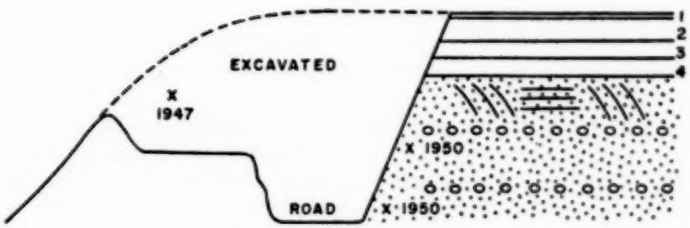
ARTIFACTS FROM THE SAN DIEGO AREA



ARTIFACTS FROM THE SAN DIEGO AREA



a



1. 2" SOIL FORMED ON OLD MIDDEN
2. 36" OLD YELLOW CEMENTED MIDDEN
3. 16" OLD METAMORPHOSED CEMENTED CLAY
4. 16" CEMENTED ZONE, HARDER THAN 3

20' UNCONSOLIDATED SANDS AND GRAVELS, SANDS OFTEN STRIKINGLY CROSSBEDDED. ARTIFACTS THROUGHOUT; LOCATIONS X.

b

FIG. 2. Location and cross-section of the Sweetwater River valley site: Nestor terrace.

tions, had developed a clay pan or, alternatively, sufficient time has elapsed, with long periods of no occupation, that the earlier midden matter became converted into a mature soil with a clay pan. Shell and worked stone extend into layer 3, the cemented layer, and appear to extend below the present level of lime cementation. There is a suggestion then that even the surface midden may have a pre-Recent beginning.

Beneath these cemented layers lie unconsolidated clean sands and gravels. The sands predominate in the upper part, are stratified, and at places strongly cross-bedded. Near road level a part of the gravel is composed of cobbles of a granitic rock. These are so rotted as to be easily smashed by a moderate blow from a geological pick. The artifact illustrated in Plate 4 c-d came from this basal layer. The others (Plate 4 b, e, f, g-h) came from stratified gravel lenses above it, but beneath a minimum of five to six feet of clean, cross-bedded sands, which in turn are overlaid by five feet of cemented midden and soil horizons.

There seems to be no possibility of the artifacts having been intruded, both because of their lateral distance back from the original terrace surface, their vertical depth from the terrace surface, the sealing effect of the cemented horizons, the clearly stratified but undisturbed very deep sands and gravels, and most importantly because of the evidence of their having been worn by stream action prior to their deposition.

The artifacts themselves are simple forms, and rolled. The question of their being natural or artificial is a real one, to which particular attention has been given. Archaeologists unfamiliar with southern California stone work always question the validity of early La Jolla artifacts since most of the tools consist solely of flakes and cores. It is always possible in the field to demonstrate that fire-broken hearth stones do not produce fractures similar to percussion fracture, and that beaches and stream beds do not produce flaked stone, except where erosion of adjacent occupation areas supplies them. Examples of natural wear on artifacts from known sites are illustrated in Plate 5. Figures d, e, f are from the cobble beach at the Scripps Institution of Oceanography and represent varying stages of wear typical of violent surf action. Artifacts are supplied to this beach from an eroding midden immediately above it. It is notable that there is no evidence of fracturing or flaking after manufacture but only of smoothing and polishing. The type and degree of wear shown in Figure e is reproduced on blades of very similar stone from the beaches of lakes in the Mohave desert.[9] A further example of moderate wear on an artifact is represented in Plate 5 g.

9 M. J. Rogers, *Early Lithic Industries* . . . (San Diego Museum Papers, no. 3, 1939), Plate 6, e.

This bifaced core tool probably belongs to the San Dieguito culture and was carried not more than one-half mile down the bouldery bed of the San Dieguito River. Its degree of wear is comparable to the artifacts from the Sweetwater River.

Plate 4, Figures c and d give two views of a retouched flake. The surface illustrated in c is a flake surface, that is, a surface created by a percussive fracture. Subsequent to the creation of this surface, a series of blows have been struck from the natural surface along two opposite edges of the artifact. It is the localized, controlled nature of these blows which seems conclusive. They represent directed, as opposed to random, blows. Man strikes directed blows and creates sharp edges. Nature strikes random blows and creates rounded forms, as is evidenced by the characteristic beach and stream cobble, and by what nature does to sharp-edged artifacts, as discussed above and illustrated in Plate 5.

Figures g and h in Plate 4 give two views of a stone which again presents evidence of deliberate fracture. A plane surface was created by splitting a boulder which seems to have had at least one large flake struck from it prior to the detachment of this piece. Thereafter, at least three blows were struck along one edge from this prepared platform.

Figure b in Plate 4 shows no retouching but does show an extensive series of fracture surfaces; only one face of the stone now retains the original cortex. How different this is from natural action can be appreciated by again referring to natural gravels of streams and beaches. Figures e and f represent artifacts collected in 1947 before the borrow pit had cut so deeply into the terrace. These artifacts came from near the base of the cross-bedded sands. Again, the multiple fracture surfaces indicate human rather than natural work. The conclusion is advanced that artifacts were being eroded by the Sweetwater River from a camp site, carried some relatively short distance (as evidenced by the moderate wear) and deposited in this valley fill. Since the valley fill has an elevation of fifty feet and this location is at the mouth of the river, it is concluded that this is a last interglacial valley fill referable to Ellis and Lee's Nestor terrace, and the contained artifacts are of that age.

VALLEY ROAD

A branch canyon entering the Sweetwater River on the north side from the Lincoln Acres exposes a sharp contact between the Pleistocene and the older formations here. These gravels also contain rolled artifacts in place in the stratified materials. A large retouched flake from these gravels is illustrated in

Plate 4, Figures i and j, while Figure a illustrates a core from this same site. Both the fracture surface of the flake and the retouched edges indicate that this is an artifact. Such artifacts are common in La Jolla period sites. The large care is more heavily worn, but is characterized by fracture surfaces with flaked edges.

The top of the valley fill here approached one hundred feet. The capping material is weathered to reddish tones, indicating humid conditions—the migration of the iron family accompanying humid weathering—with the formation of the red clays characteristic of mature humid soils in general and in this area in particular. Since this site is two and a half miles from the present bay margin, the height of its fill does not represent sea level of its time of deposition. It is clearly higher than the 25 foot terrace which is well represented about the mouth of Paradise valley, which joins the Sweetwater valley at the bay. It would seem then to belong to an earlier third interglacial level comparable to the Sweetwater River site described above.

THE GRAVEL PIT AT THE EMORY SCHOOL

The area to the south of San Diego Bay is composed of broad low terraces. This area was studied with particular interest because it is the type locality for Ellis and Lee's Nestor terrace. It soon became apparent that the landforms were compound rather than simple and that two or more terraces were included in the area. Examination of a large borrow pit just west of the Emory School showed that the terrace materials were closely similar to those comprising the Sweetwater valley fill and were capped by similar soils. The materials seen by me are non-fossiliferous and because of the sharpness of the sand grains and subangularity of some of the included gravels it would seem that at least the upper part of this deposit is riverine rather than marine. The borrow pit is in the higher part of the area in a part of the terrace that is more than 50 feet above sea level. A lower terrace lies at about 25 feet and probably belongs to the Tia Juana terrace of Ellis and Lee's terrace system. The gravels at about 50 feet clearly belong to the same time period as the deposits described above in the Sweetwater River.

The artifact illustrated in Plate 5, a and b, was found in the borrow pit in the Nestor terrace. It lay at an unknown depth from the original surface, which had been removed, but was solidly in place in the base of the indurated materials which normally cap these Pleistocene alluvial features, as has previously been described at the San Diego Mission site and at the Sweetwater River site.

Since the depth of weathering on these terraces is normally about five feet, it is probable that no more than four feet of material had been stripped off. The rounding of edges and general smoothing indicates that this artifact was subjected to stream wear. The flake surfaces are slightly altered and are coated with some amorphous material, perhaps silica. That this is a core from which flakes have been struck is evident from the nature of the fractures, which are the result of tremendously heavy percussive blows, and from the fact that a number of such heavy blows were so struck as to detach a series of large flakes without battering any of the previously created sharp edges. The degree of wear on this artifact is comparable to that on Plate 5 g. But while the maximum probable movement of this specimen is one-half mile, the minimum distance to a source of cliffs for such a boulder as this to fall off and become so fractured in nature is more than ten miles. A comparable flake core from a late pre-pottery La Jolla culture site is illustrated in Plate 5 c for comparative purposes.

CONCLUSION

No assemblage of artifacts such as this can be very meaningful in defining an industry or culture. What has been recovered are random finds too few in number to allow much generalization. The material presented shows only simple percussive flaking to create unspecialized tools. What sort of pattern will emerge, if and when more artifacts are found in these and comparable deposits, would seem difficult to predict, except with reference to other finds. If the geomorphic-pedologic age suggested for these materials is correct, it would be somewhat surprising if blades and other developed tools were to be found. As has been pointed out elsewhere, there are lithic industries that seem to be limited to such simple artifacts as this,[10] and Treganza's[11] recently reported materials may possibly prove to be of related culture level.

This is, then, a long delayed preliminary report on artifacts of probable third interglacial age. Other reports embodying the extensive mapping of terraces and valley fills, their associated soil types, the geochemical proofs of the relic nature of the soils (in preparation by V. P. Sokoloff) has been long, arduous, and frequently interrupted. This advance notice must serve till time allows a

10 G. F. Carter, *Man in America: a Criticism of Scientific Thought* (Scientific Monthly, vol. 73, no. 5, pp. 297-307, 1951).

11 A. E. Treganza, *Archaeological Investigations in the Farmington Reservoir Area, Stanislaus County, California* (Reports, University of California Archaeological Survey, no. 14, 1952).

fuller reporting of these collateral materials. It is hoped that this report will lead others in the region to check these and comparable sites, and those in other areas to examine more closely the interglacial deposits of their region. For, it is certain that if interglacial man was in America, he did not confine himself to southern California.

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TIME PERSPECTIVE IN MICRONESIA AND POLYNESIA

ALEXANDER SPOEHR

OCEANIA has been a favorite locale for those who have sought to reconstruct the sequence of past events on the basis of ethnographic evidence alone. Devoid of documented history prior to European contact, with until recently no archaeological data of material aid, Oceania presented to the student of culture history only a one-dimensional level of regional ethnology. The rigorous handling of the ethnographic data from this area, when combined with the evidence of linguistics and physical anthropology, has indicated much as to the probable direction of migration, and the general course of culture history in later periods. Nevertheless, little demonstrable chronology has thereby been established, while the more ambitious and speculative attempts at historical reconstruction are as complicated as they are improbable. Although important work of real significance for prehistory remains to be done in blocking out the contemporary distribution of languages, physical types, and cultures, particularly in Melanesia, further progress in unravelling the prehistory of Melanesia, Polynesia, and Micronesia is primarily dependent on the prosecution of carefully planned archaeological research. Only archaeology will provide sequences of culture change in Oceania that can engage the confidence of the critically minded.

The present paper is an outgrowth of recent archaeological work in Micronesia and Polynesia and deals only with those two areas. The following discussion treats two questions: (1) Do the established divisions of island Oceania provide the most useful framework within which to view the problems of Micronesian and Polynesian prehistory? and (2) On the basis of archaeological evidence what glimpses into the past do we possess, and what are some of the major problems that lie ahead?

THE AREAL FRAMEWORK OF MICRONESIAN AND POLYNESIAN PREHISTORY

The division of the island area of Oceania into Melanesia, Polynesia, and Micronesia developed out of 19th century usage until today it forms the accepted framework for describing the peoples and cultures of the great stretches of Pacific to which it refers. It is largely within this framework that students of Oceanic prehistory have attempted to reconstruct the past movements of

peoples, the diffusion of culture, and the development of regional culture growths.

That there are significant racial, linguistic, and cultural differences between Micronesia and Polynesia is evident. There are, however, other considerations that emphasize resemblances rather than differences. In technology, in social organization, in mythology, and in art there are basic similarities as well as differences. Nor does there seem to be a sharply marked boundary between the two areas. With the dearth of published material on the Gilbert and Ellice groups, this question cannot be resolved until the recent research by Luomala, Hughes, Goodenough, and others appears in print, but it is likely that along their common border Micronesia and Polynesia show a gradual merging of their major culture patterns and not a major break. A detailed examination of these patterns is beyond the scope of this paper, but it is suggested here that Micronesia and Polynesia taken as wholes are more closely similar to each other in the physical type of their inhabitants, in language, and in culture than either is to Melanesia.

Another point important in the setting of historical problems is that both Micronesia and Polynesia could only have been peopled by voyagers possessing a developed form of water transport. This point has been made repeatedly in the past but needs constant emphasis. Unlike the New Guinea area, the islands of Micronesia and Polynesia throughout human history have always been separated from each other and from continental land masses by great expanses of ocean. Only a people with skill in the building and sailing of canoes suitable for open-sea transport could negotiate the required distances. Both Micronesians and Polynesians possessed these skills and the necessary knowledge of the sea. Both Micronesia and Polynesia were inhabited by intrepid sailors, and both areas were peopled by a major movement out of the general Malaysian region, Kon-Tiki notwithstanding. But it is important to remember that this movement could not have taken place without seagoing canoes.

The significant resemblances between Polynesia and Micronesia, the important requirements imposed by the island environment of both areas for a developed form of water transport, the likelihood that both Polynesians and Micronesians were part of the same general movement eastward from Malaysia raises the question as to the most useful spacial framework within which to view the problems of Micronesian and Polynesian prehistory. It is suggested here that our perspective on these problems would be clarified if we avoided a rigid separation of the two regions and for purposes of historical analysis combined Micronesia and Polynesia into a single major area. A satisfactory term for

such an area is difficult. *Micro-Polynesia* seems as suitable as any, and is herewith proposed.[1]

The combining of these two major regions, it should be emphasized, is suggested in order to provide a larger framework for the interpretation of historical events and for defining broad historical problems. *Micro-Polynesia* is proposed in a tentative way; its ultimate acceptance or rejection will depend on its usefulness. It is not intended to displace for purposes of ethnographic description the terms Micronesia and Polynesia but is rather intended to provide a supplementary and at the same time a more inclusive framework in which to fit large-scale temporal sequences and spacial relations.

From the knowledge at hand, it can be proposed that prehistoric events in Micro-Polynesia can be characterized by their expansive force. It seems likely that the initial stimulus to the migrations into Micronesia and Polynesia from the Asiatic area was the pressure of Mongoloid peoples behind the Micro-Polynesians. But it is doubtful that such a fact can explain entirely the extent of these migrations. The expansive force that lay behind the migrations eastward to the farthest islands of Polynesia must have been partly a cultural characteristic reflected in a seafaring tradition. Enough of this was still present by the time of European contact to be appreciated by Westerners.

The larger and more inclusive framework of Micro-Polynesia and the expansive force characteristic of its prehistory also assists us in viewing in a useful way the expansion of the Micronesians and Polynesians into Melanesia. At one time the so-called Polynesian outlyers along the eastern fringe of Melanesia were regarded as evidence for the migration of the Polynesians through Melanesia. Today it is considered more probable that they represent a backlash from Polynesia westward. In addition, the northern outlyers also incorporated Micronesian culture elements brought down by voyagers from the north, and there has been discussion as to whether the northern outlyers should be classed primarily as Micronesian or as Polynesian.[2] Actually movements into these outlyers from both Micronesia and Polynesia can be viewed as manifestations of the same expansive force that motivated both Polynesians and Micronesians—the settlers of Micro-Polynesia. In this same current of prehistory can be fitted the Mi-

1 The paucity of color adjectives in Greek makes the coining of a word contrasting to *Melanesia* very difficult. *Xanthonesia* is a possibility, but *xantho* in translation has come to mean yellow, and has been confusingly used in the earlier anthropological literature. The writer is indebted to Professor Francis R. Walton of the University of Chicago for advice on this point.

2 See particularly H. I. Hogbin, "'Polynesian' Colonies in Melanesia" (in *Polynesian Anthropological Studies*, Memoirs, Polynesian Society, vol. 17, Wellington, 1941).

cronesian outlyers of Aua and Wuvulu off the north coast of New Guinea and the evident Micronesian influence in the nearby Ninigo and Hermit islands. Possibly, Polynesian-like influences in the large islands of eastern Melanesia are due to this same flow of events.

I am aware that grouping Micronesia and Polynesia together in a larger area for purposes of historical analysis assumes, implicitly at least, that the main migration route of the Polynesians was through Micronesia. This I believe to have been the case, at the same time not discounting probable migrations of Micro-Polynesian peoples along the southern route into Melanesia. Melanesia as a whole, however, gives the impression of being a receiving and importing area, and more important as a point of destination than as a way stop.

TIME PERSPECTIVE AND ARCHAEOLOGY

Within the areal framework of Micro-Polynesia, what time perspective has the archaeologist provided? Until recently Micronesian and Polynesian archaeology has revealed little in the way of culture differences between archaeological manifestations and the historic cultures, while it has been further handicapped by the lack of an absolute chronology, other than the approximations offered by Polynesian genealogies. In the last few years, however, the beginnings of a chronology are emerging. In its further development radiocarbon dating must play a decisive role.

At the time of writing four Carbon 14 dates from Micro-Polynesia are available.

1. AD 1005 ± 180 from Oahu, Hawaii (excavations by Kenneth P. Emory)[3]
2. AD 1002 ± 300 from Viti Levu, Fiji (excavations by E. W. Gifford)[4]
3. 1527 BC ± 200 from Saipan, Marianas (excavations by Alexander Spoehr)[5]

AD 845 ± 145 from Tinian, Marianas (excavations by Alexander Spoehr)

3 W. F. Libby, *Radiocarbon Dates, II* (Science, September 21, 1951, p. 295).

4 E. W. Gifford (personal communication, March 28, 1952). The author is indebted to Professor Gifford for permission to refer to this date, which was unpublished at the time of writing this article. The date was determined by Professor H. R. Crane of the University of Michigan Memorial-Phoenix Project Radiocarbon Laboratory. A note by Gifford on the Fiji date will appear in the Journal of the Polynesian Society.

5 W. F. Libby (personal communications, February 11 and September 1, 1952). The author is grateful to Professor Libby for making the radiocarbon determination of the Marianas dates. The Marianas excavations were conducted during 1950 for the Chicago Natural History Museum, with the sponsorship of the Pacific Science Board, and with the coöperation of the United States Navy.

Whether the date from the Oahu rock shelter marks the first peopling of the Hawaiian Islands must await Emory's considered conclusion and the publication of his report on his highly significant findings. The date of the Fiji sample is contemporaneous with that from Oahu, but in a personal communication to the writer, Gifford has pointed out that the sample does not come from his lower levels of excavated cultural material. These lower levels are very considerably earlier. I would not be surprised if it is eventually demonstrated by Carbon 14 dating that Fiji was already settled by at least the beginning of the Christian era.

The third date was obtained from a large oyster shell excavated at the Chalan Piao site on the west coast of Saipan. The site is located approximately 2000 feet inland in undisturbed indurated sand beds. On the surface were scattered sherds of Marianas plain ware, commonly associated with the upright stone columns, or *lattes,* that are the dominant feature of archaeological sites in the Marianas, though no evidence of *lattes* or other structures was found at Chalan Piao. For a depth of 5.4 feet below the surface, sherds were found scattered through the sand beds. The oyster shell was discovered at a depth of 1.5 feet, associated with sherds. The shell showed no signs of rolling, and it is probable that only a short interval elapsed between the time the oyster died—probably eaten by man—and the deposition of the shell.

The geological history of the indurated sand beds in which the site lay is being worked out by Dr Preston E. Cloud of the United States Geological Survey: his conclusions will appear in due course.[6] Here it may be noted that the deposition of the sand beds is probably related to eustatic shifts in sea level associated with the post-glacial optimum. How the oyster shell and sherds came to be where they were found is not entirely clear as yet, but they may well have been deposited at the toe of a sand spit on which a camp site was located. On the whole, the geological evidence indicates that the Carbon 14 date is, if anything, a minimum date for the deposit.

It should also be noted that below the oyster shell were some four feet of deposit containing sherds. The levels below the shell were characterized by the presence of a red-slipped, thin walled pottery markedly different from the Marianas plain ware on the surface of the site. Assuming the oyster shell was deposited not long after its death, the sherds lying below it are of considerably

6 The assistance and interest of Dr Cloud in the archaeological investigations on Saipan is gratefully acknowledged. The writer is also indebted to Mr Harold W. Burke and other members of the United States Geological Survey for their coöperation in the field.

greater age. On the basis of the Carbon 14 date, it is probable that man reached the Marianas by 2000 BC, if not earlier.

The fourth Carbon 14 date was derived from a tridacna shell excavated from a small refuse mound, associated with a *latte* type house. Documentary evidence indicates that *latte* type houses were in use at the time of European contact. The chronological significance of the Tinian date is that it is the first evidence we have for determining the time-span covered by the characteristic culture associated with Marianas *lattes* and demonstrates a marked degree of cultural stability, as reflected in archaeological remains, for some eight centuries.

These four Carbon 14 dates are not sufficient to establish a chronology for Micro-Polynesia, but they are adequate for formulating a series of working hypotheses. Certain of these radically alter our conceptions of the prehistory of Micro-Polynesia.

The first hypothesis is that by 1500-2000 BC, a form of seagoing transport had been developed in the Malaysian-Southeast Asian region sufficient to carry man into western Micronesia. From the widespread distribution of the single outrigger canoe in Micronesia and Polynesia, it is a good possibility that this was the type used to carry these early voyagers to the shores of the Marianas. However, the presence of large double canoes in Polynesia and the problem of their antiquity serves to complicate the question. For the sailing outrigger, the only direct evidence of its use in ancient times of which I am aware are the well-known representations sculptured on the walls of Borobudor in Java and dating from the eighth to ninth century AD.[7] This is not particularly old. If the early settlers in the Marianas used the sailing outrigger, then it had been developed for open sea transport by at least 1500-2000 BC, if not considerably earlier.

The second hypothesis deriving from the three Carbon 14 dates is that the eastward migrations into and through Micro-Polynesia took place over at least a two and a half thousand year period. The terminal points of this two and a half millenium chronology are set by the Saipan and Oahu dates, and to this large period must be added the increment of time between 1000 AD and the point of first European contact.

The nature of man's expansion eastward is still unknown. Whether it occurred as a slow trickle or as a series of bursts or waves of migration is at present pure guesswork. Polynesia may still have been reached only relatively

7 T. Van Erp, *Voorstellingen van Vaartuigen op de Reliefs van den Boroboedoer* (Nederlandsche-Indie, Oud en Nieuw, The Hague, 1923); James Hornell, *Water Transport* (Cambridge, 1946).

recently. But eventually we shall be in a better position to reconstruct the character of this migration and to relate it to the concept of expansive force that has been suggested here as a primary characteristic of Micro-Polynesian prehistory.

How man, sailing out from the Asiatic area, was able to locate the islands dotted over the great expanse of the Pacific in Micronesia and Polynesia is still somewhat of a puzzle. As at least a partial explanation, it has been suggested that he followed the flyways of shore birds.[8] These flyways could well have been a critical factor in the first major jump, to the Palaus and to the Marianas. The flyways of Micronesia have recently been described; there are two of particular importance.[9] The Asiatic-Palauan flyway, which runs through the Philippines, would have been of material aid in locating the Palaus. The Japanese-Marianas flyway extends southward to the Palaus and could have been used in navigating northeastward from the Palaus to the Marianas. The utility of flyways as navigational aids deserves further detailed study.

Perhaps the most important question raised by the available Carbon 14 dates, however, is: What culture change took place within the approximately three thousand year period of the present fragmentary Micro-Polynesian chronolgy? The answer to this question must await the determination of local chronologies of culture change for individual island groups. At present, important published material has recently come from New Zealand, with Duff's description of the Moa-hunter period antedating the historic Maori.[10] Here a distinct culture change is evident, which it is most desirable to date by Carbon 14.[11] The publication of Emory's report on his dated Oahu excavation is awaited with great interest, as preliminary news from Hawaii indicates he has found discernible culture change covering the period from the probable first settlement to historic times.[12] In Fiji, on the border between Polynesia and Melanesia, Gifford has described a sequence of change, principally with respect to pottery types.[13] He is now engaged in what promises to be a very significant attack on Melanesian pre-

8 Peter H. Buck, *Vikings of the Sunrise* (New York, 1938), p. 136.

9 Rollin H. Baker, *The Avifauna of Micronesia, its Origin, Evolution, and Distribution* (University of Kansas Publications, Museum of Natural History, vol. 3, no. 1, 1951).

10 Roger Duff, *The Moa-Hunter Period of Maori Culture* (Canterbury Museum Bulletin, no. 1, Wellington, 1950).

11 Ralph Linton, *Review of Duff, op. cit.* (American Anthropologist, vol. 53, 1951), p. 264.

12 Peter H. Buck, *Report of the Director for 1950* (Bulletin, Bernice P. Bishop Museum, no. 205, 1951), p. 7.

13 E. W. Gifford, *Archaeological Excavations in Fiji* (Anthropological Records, University of California, vol. 13, 1951).

history by extending his work westward to New Caledonia. In the Marianas, there are also demonstrable differences in pottery types between the earliest occupations and those of the builders of the stone columns (*lattes*). For the earliest period, however, little is known except the pottery, and further excavations are necessary before anything as pretentious as a "culture" can be delineated.

The important fact about these excavations is that discernible culture differences through time can be found in Micro-Polynesia, admitted that the job is not easy. The experience of anthropology tends to show that culture change, however slow and however isolated the group, is bound to occur. Some of this change generally is reflected in the artifacts the archaeologist unearths. Micro-Polynesia is no exception, granted that until recently the results of archaeological work have been somewhat discouraging in this regard.

Nevertheless, that radical differences in culture type through time will be delineated for Micro-Polynesia as a whole seems doubtful, because there is little evidence they ever existed.[14] It seems most likely that the first voyagers brought with them the principal adaptations that were at the core of the historic cultures. A developed form of water-transport was essential. The tropical root crops—taro and yams—may well be among the oldest of cultivated food plants and one suspects were brought by the first migrants. Likewise the banana and the breadfruit, not to mention the coconut, may likewise have been dispersed very early. Although Duff believes the Moa-hunters of New Zealand were pre-agricultural, it seems doubtful that a pre-agricultural stage was common throughout Micro-Polynesia.

The island environment itself had decidedly limiting influences. Large concentrations of population acting as points of continuous diffusion and interaction over a major region were virtually impossible, at least in contrast to continental areas such as Meso-America or the Near East. Despite their facility with water craft, the Micro-Polynesians must have found the ocean a barrier. Diffusion could not take place, of course, without actual migration in this area, except locally, and even major island groups must have experienced considerable periods of isolation.

The foregoing remarks, largely speculative, have served to indicate that archaeology in Micro-Polynesia is beginning to open new vistas into the past. For maximum productivity of future archaeological work in this area, it is most desirable that it proceed with a sound over-all strategy. Of utmost importance

14 Cf. Kenneth P. Emory, "Polynesian Stone Remains" (in *Studies in the Anthropology of Oceania and Asia*, Carleton S. Coon and James M. Andrews, IV, eds., Papers, Peabody Museum, Harvard University, vol. 20, 1943).

is the establishment of a framework of Carbon 14 dates from major island groups, for radiocarbon dating is at present the only available basis for absolute chronology in the area. Whatever archaeologists do, or are supposed to do, their first job is establishing temporal and spacial relationships. This is the immediate task ahead in Micro-Polynesia. The selection of particular islands for survey and excavation must be based on this consideration.

For Micronesia, an outline of recommendations as to strategy has been formulated.[15] For Polynesia, the anthropological work and discussion of the last half century serve to indicate the critical spots: Samoa, Tonga, and Fiji in the west; the major islands of central Polynesia; and the principal marginal groups such as Hawaii, New Zealand, and the Marquesas. On these islands a background of archaeological work already has been achieved that provides a sound basis for the future.[16] For Micro-Polynesia as a whole, the outlook is favorable for the fruitful exploration of its prehistory. It need not be considered a region whose past can be reconstructed only from the examination of the "ethnological present." On the other hand, the probable rather narrow range of cultural variation through time in Micro-Polynesia in contrast to major continental culture growths, and the basic similarities so far uncovered in archaeological and historic cultures make ethnographic facts essential in archaeological study. One can predict that the future course of research in Micro-Polynesian prehistory will see the development of an approach of proven and growing significance in anthropology—the full coördination of the methods of archaeology and historical ethnology in a joint attack on common problems.

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15 Sub-Committee on Pacific Archaeology, National Research Council, *A Program for Micronesian Archaeology* (American Anthropologist, vol. 53, pp. 594-597, 1951).

16 See particularly reports by Wendell C. Bennett, Kenneth P. Emory, Ralph Linton, J. G. McAllister, and W. C. McKern in the Bulletin series of the Bernice P. Bishop Museum; Duff, *op. cit.*, and Gifford, *op. cit.*

LANGUAGE AND CULTURE: A COMMENT ON VOEGELIN'S VIEW

WILLIAM E. BITTLE

IN A RECENT ARTICLE,[1] C. F. Voegelin, for the second time in two years, addresses himself to the proposition that there is to be distinguished a major difference between language and culture in at least a definitional sense, and that an attempt to include both phenomena in the same definitional frame, and, apparently, in the same conceptual frame, meets either with failure or loss of methodological rigor. The argument to support this thesis takes form in a series of paragraphs which illustrate (in terms of a potpourri definition of culture) that one can simply not *say* the same things about language and culture.

The definition (all definitions of culture, we are led to believe) begins with behavior. "Whatever else is included can be designated as successive or occasional restrictions,"[2] which may be stated as follows:

(1) learned behavior;
(2) behavior which is transmitted, or transmissable, from one generation to the next;
(3) diffusable behavior;
(4) behavior supervised and directed by human beings;
(5) not only behavior thus restricted, but also "statements about such behavior abstracted by persons who may be outside the society investigated";[3]
(6) the actual behavior of participants in the culture.

Despite the fact that this series of restrictions—which are said to constitute some sort of definition, or at least represent characteristics of most (or all?) definitions—are drawn with an eye to their inhospitality for the inclusion of language, and from authors spread over fifty years of time and with diverse theoretical positions, we may find it illuminating to examine each, in the terms which Voegelin himself proposes, and then in other terms. It is my contention that the attempted application of these restrictions to language and culture represents not only a fallacious method of analysis, but is simply incorrect in that the level

1 C. F. Voegelin, *Culture, Language and the Human Organism* (Southwestern Journal of Anthropology, vol. 7, pp. 357-373, 1951).
2 *Idem*, p. 365.
3 *Ibid.*

at which such analysis is carried out is allowed to shift as one moves from a discussion of language to a discussion of culture. Only the latter point, however, will be discussed here.

Of the first two restrictions, and their applicability to both phenomena, Voegelin says

> The first and second restrictions apply to language as well as to culture—one might just as well say, then, to language and the rest of culture.[4]

The other four restrictions, however, do not apply. With respect to the third, we are told that "culture elements and culture complexes—and these in massive doses—diffuse readily from society to society."[5] The evidence which one may obtain from maps of culture areas, with their indefinite boundaries between "dissimilar" cultures attest to this. But what of linguistic areas? Here we find the fuzzy boundary a rare exception to the rule of clear-cut demarcation. Language, we are told, does not readily diffuse—in doses massive or modest. Whatever diffusion does take place is limited to vocabulary items, and even here, "diffusion is best stated in terms of open morpheme classes in particular languages . . . which are hospitable to borrowing words."[6]

It is obvious that these statements simply miss the point. The examples selected as illustrative are not of the same, or even similar, order. If we wish to find an analogue in non-linguistic culture for such complexes in language as grammatical systems, we do not look at the level of the hand-axe and the sewing machine. More realistically, we examine behavior patterns which are actualizations of such large-scale cultural configurations as value systems. The latter are notoriously inhospitable to diffusionary processes. It is just as inconceivable that a philosophy of self-denial would diffuse easily to the United States as that an aspectival system of verb inflections would suddenly be incorporated into English. If we examine, even superficially, those aspects of culture which Voegelin alludes to when he talks of culture complexes, by which I take him to mean more than the automobile and related accessories, we will find no "doses" at all in process of diffusion.

We certainly have, on the other hand, evidence of a wholesale acceptance by a native people of an alien culture complex. But just as frequently (and, of course, often concomitantly) a native people learn an alien language.

A digging stick, the proper analogue to a vocabulary item, will diffuse easily under proper circumstances. It is also true that subsequent changes in the culture

4 *Ibid.*
5 *Idem*, p. 366.
6 *Ibid.*

may be expected. But these are of no greater (or lesser) significance than the changes wrought in the phonemic system of a language by the adoption of a foreign lexical item. If we seek analogous items from the linguistic and non-linguistic culture, we find that this restriction in no way excludes language.

The fourth restriction derives from Bidney's notion of the anthropoculture. Voegelin cites the following: ". . . every stage of the anthropocultural process is supervised and directed. . . ."[7] We are led to believe, since no other information is offered, that Bidney's anthropoculture is nothing more or less than the old familiar anthropological notion of culture. However, when we examine the fuller context, we obtain quite another impression. Bidney says, in full

> The notion of culture is best understood from a genetic and functional point of view. To cultivate an object is to develop the potentialities of its nature in a specific manner with a view to a definite end or result. Thus, for example, *agriculture* is the process whereby the potentialities of the earth and of seeds are cultivated and brought into relation with one another with a view to the growing of edible plants. In like manner, *anthropoculture,* as it may be called, "comprises the various ways in which man has tended his nature so as to make it grow." But human culture differs from agriculture in that every stage of the anthropocultural process is supervised and directed with a view to producing a type of man and society which is adjusted to its geographical and social environment biologically as well as intellectually. Anthropoculture so conceived refers to the dynamic process of human self-cultivation and is identified with education. From a historical point of view it is easy to understand why, as men came to attach greater importance to the cultivation of their mental nature or "soul," the term "culture" came to refer specifically to the latter, and culture became identified with *cultura animi.*[8]

Whatever else may be suggested by this passage, we are reminded more of Culture than of culture. The anthropoculture is identified with education and with man's self-cultivation. In these terms, then, it is not easy to understand why language may not be included in a definition which contains this restriction. We have the entire literature of a people to attest to the fact that man constantly directs his language by "enriching" it with novel locutions and extraordinary terms. Again, the coining of special vocabularies in the sciences, or the invention of logical systems (e.g., a propositional calculus) are evidences of the supervising and directing of a language by the people who speak it.

In the non-linguistic culture, social reforms are similarly instituted as purposive acts of men. But the extent to which these innovations effect either the

7 *Idem,* pp. 366-367.

8 David Bidney, *Human Nature and the Cultural Process* (American Anthropologist, vol. 49, pp. 375-399, 1947), pp. 375-376.

language or the culture of a people is not profound. This kind of purposive alteration of certain behavior patterns is a far cry from any grand-scale change in culture or language. Such changes are operative only within the broader, and vastly more rigid, contexts of language and culture, and the nature and direction of change must be regarded as in large part determined by these contexts.

The fifth restriction allows for a possible confusion. When we say that a grammar of a language says nothing at all about the cultural context in which the language is spoken, we may be saying one of at least two things. It has been suggested in recent years (though never extensively demonstrated in practice) that a description of language may be accomplished without any reference to meaning. By this is meant that not even form equivalence is admissible as a criterion in the determination of the phonemes and morphemes of a language, since this equivalence is inevitably couched in terms of meaning. Two forms are said to be equivalent if they mean the same thing to a native speaker of the language. Two forms are said to be not equivalent if they do not mean the same thing to a native speaker (i.e. if they contrast in meaning). If it is possible to write a grammar of a language without reference to meaning, utilizing features of randomness of occurrence in particular environments, and so forth, as criteria, we would certainly be able to infer very little from the grammar about the culture in which this language is spoken. But such a grammar would say very little of interest about the language. It would simple list a series of forms. We could not talk about verb categories (except in purely structural terms) nor about aspects, modes, and tenses, since these all depend on meaning for definition. But this is not, I assume, the kind of grammar about which Voegelin is writing. I take him to mean the conventional grammar, which does assign meaning, lexical and structural, to categories of forms. If this is the case, then it is incorrect to say that an investigation of such forms tells us nothing about culture.

It has been argued that a complete lexicon of a language gives us an accurate ethnography of the people who speak that language. Further, grammatical categories, we are told, present to us in capsule form the *Weltanschauung* of a people. We are able, on careful inspection, to determine what kinds of categorizations are made of things in the perceptual and experiential world. We can infer from language how a people conceive of time and space, their notion of the universe and their relation to it. If this material is not cultural data, then it is difficult to imagine what is.

Similarly, beginning with cultural constructs of the type cited by Voegelin, we obtain hints as to what to expect in the language of a people. To be sure, we cannot predict a particular morphological process. But the Navaho language may

come as no surprise to a person who has studied Navaho culture. The Wintu regard for detailed information as to the source of a statement, and the evidence upon which it is based, is detectable in both the linguistic and cultural material. These cross-predictions have not, of course, great accuracy. We clearly gain a more accurate and thorough picture when linguistic and non-linguistic materials are studied simultaneously. But the fact remains that there are demonstrable correspondences, and this in itself casts doubt on Voegelin's statements to the contrary.

Finally, Linton's distinction between "real culture" and "ideal patterns" is entered as evidence. Voegelin feels that it is "unlikely . . . that any parallel to 'ideal patterns' would be found in linguistic investigations, for speakers are notoriously unconscious of what they do when they speak. . . ."[9]

It is difficult to imagine that speakers of a language are more unconscious of what they do when they speak than are participants in a culture when they behave. Again, we detect a shift in levels. The patterns of behavior which guide individual actions cannot be regarded as consistently present as formulated axioms in the consciousness of the participants in a culture. We not only engage in many diverse activities, totally unaware of our actions, but are further quite able to meet unique situations with all of the assurance with which our "non-conscious guides" invest us. New situations are reacted to in terms of old situations; the guides which are effective in an inter-personal relationship with A (whom we have known for ten years) are generally equally effective with B (whom we have just met). Similarly, in linguistic behavior, we can say, "There is ice-cream on my beard," whether or not we have ever had occasion to say this before. We can, in short (to borrow Voegelin's term) substitute in a frame in matters cultural as well as linguistic. But these are the patterns of the "real culture."

What of the "ideal patterns"? It would be idle to regard the latter as unique in the complex of social behavior. They are as integral in the cultural context, and as automatic in broad behavioral terms, as are patterns of the "real culture." Our internalization of "ideal patterns" is a part of the learning process just as much as our acquisition of "real culture" patterns.

In linguistic behavior, the presence of "ideal patterns" can scarcely be denied. "Is not" is certainly preferred to "ain't" in a number of speech communities. But the frequency of "ain't" may be only slightly altered in speech. But just as "ideal patterns" of non-linguistic behavior are formalized in homilies and stories, "ideal patterns" of speech are formalized in traditional grammatical rules (or

9 Voegelin, *op. cit.*, p. 367.

"non-traditional" rules, for that matter) and in the "proper usage" recognized by a speech community. It is certainly unlikely that there exists a speech community on the face of the earth in which the members do not correct the speech of their offspring, just as they correct the behavior of their offspring. If this were not the case, the future of such a language would be hazardous indeed.

In short, in the sense in which Linton has used "ideal patterns," there may certainly be said to be "ideal language." The iconoclasticism of modern linguistics should not be allowed to so thoroughly sweep away the notion of "correct language" as to ignore its existence on the very real level of human behavior.

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AUSTROMELANESIAN MIGRATIONS ONCE MORE

DIRK ALBERT HOOIJER

IN A PAPER recently published in this journal, Von Koenigswald[1] has replied to my paper on the evidence of Austromelanesian migrations in Malaysia.[2] It seems to me that, although he quotes me extensively, Von Koenigswald missed my point, which was that teeth alone cannot be used in evidence of such migrations to have taken place. It may well be that I have not been sufficiently clear in expressing my views on the matter, and that I shall be misunderstood by other anthropologists too. Moreover, some of the remarks contained in Von Koenigswald's article are calculated to give the impression that my approach to the problem of the big-toothed prehistoric and Pleistocene mammals in general is wrong. Therefore, I feel that I should take up the subject once more in order that no misunderstanding is possible, which is the more urgent since Von Koenigswald in his paper remarks upon certain Pleistocene mammals from Java as evidence against my "very personal and one-sided view"—that I have to have my say upon too.

I do not wish to deny that there have been migrations of Man over Malaya and the Greater Sunda Islands toward the east. There is admittedly some evidence of such migrations in archaeological objects as well as in structural characters of the human skeletal remains,[3] evidence that is quoted to some extent by Von Koenigswald, and with which I am familiar. However, it is my opinion that most authors who have dealt with the subject of the early presence of Austromelanesoids in Malaysia lean far too heavily upon the large size of the teeth by which the living Australians, Papuans, and Melanesians happen to be characterized.

Furthermore, I find it somewhat difficult to believe with Von Koenigswald[4] that, circa 2000 BC, Malaya, Sumatra, and Java were, so to say, swept clean of all macrodont peoples to become repopulated by the Indonesians that live there now.

Von Koenigswald finds that any conclusion as to a reduction in size of the Malaysian human teeth in the course of time is irrelevant[5] because we are sup-

1 G. H. R. von Koenigswald, *Evidence of a Prehistoric Australomelanesoid Population in Malaya and Indonesia* (Southwestern Journal of Anthropology, vol. 8, pp. 92-96, 1952).

2 Southwestern Journal of Anthropology, vol. 6, pp. 416-422, 1950.

3 Cf. D. A. Hooijer, *op. cit.*, p. 422.

4 *Op. cit.*, p. 96.

5 Von Koenigswald, *op. cit.*, p. 96.

posedly dealing with two racially different human populations, viz., the prehistoric Austromelanesoids now pushed out to the east, and the peoples now living in Indonesia. In proof of the invalidity of the rule formulated by me, Von Koenigswald asserts that the animals from Middle Pleistocene deposits of Patjitan in Java may be either very large, or normal-sized, or even smaller than the recent. The passage in Von Koenigswald's paper I am referring to[6] runs as follows:

> For such a reduction in size there is no general rule: it might affect various species in different ways. In the rock-fissures of Patjitan, which very probably are of Upper Middle Pleistocene age, tapir and siamang are very large (one specimen of the latter above the maximum recorded for the living species); tiger and malayan bear are of normal size, while the orang is decidedly smaller than either the Lower Pleistocene form from Sangiran, the prehistoric form from Sumatra, or the living species from Sumatra and Borneo.

An analysis of the above quoted statements, however, leads to a totally different conclusion. Everything is quite in harmony with the rule of microevolution in situ with diminution in size that I formulated in my paper, as will appear from what follows.

The study of a number of species of Malaysian mammals has convinced me that the recent subspecies evolved in situ from subspecifically distinct populations that already existed in the Pleistocene. In a certain number of living species that are distributed over Malaya, Sumatra, and Java, the continental subspecies is the largest, the living Javan subspecies is the smallest, and the living Sumatran subspecies is more or less intermediate in size. When we trace the history of each of these subspecies back in time in its own area by means of prehistoric and fossil remains so far as available, we find as a general rule that the specimens are larger the older the deposit they are from. Thus, there is seen a gradual diminution in size during the Quaternary, a general reduction in size in situ that, of course, affects "various species in different ways" but that nevertheless is ubiquitous.

Von Koenigswald remarks that the Middle Pleistocene Patjitan tapir and siamang are "very large," without stating in comparison with what. Both the tapir and the siamang gibbon are extinct now in Java, and consequently we cannot compare the Javan fossils with material of the same "racial group." In this case the recent material for comparison must come either from Sumatra or from Malaya or Siam, hence of subspecies that have their own history behind them, separate from that of the Javan forms way back into Pleistocene times. I found the prehistoric tapir of Sumatra to be decidedly larger than the living tapir of

6 *Idem*, p. 95.

the same island; in this case there even is hardly any overlap in the dimensions of the subfossil and the corresponding recent teeth.[7] Von Koenigswald's remark can be interpreted only thus, that the Middle Pleistocene teeth of the tapir from Java are larger than those of the living southeastern Asiatic tapir. Now the prehistoric tapir from Java is represented by an isolated M^2 from the Wadjak caves[8] that keeps well in size with the recent specimens of M^2, and thus must be smaller than the Pleistocene teeth from the same island to which Von Koenigswald is referring. The Wadjak tooth also is smaller than the bulk of the corresponding teeth from the prehistoric Sumatran caves. This all tends to corroborate the evidence we have of other species which, in the course of the Quaternary, show a gradual diminution in size that is more advanced in Java than it is in Sumatra.

Tiger and Malay bear from the Middle Pleistocene or Patjitan are stated by Von Koenigswald to be of "normal size." There are, however, various living subspecies both of tiger and of Malay bear that differ in size among themselves. Javan fossils should be compared with other Javan material. Of the two species mentioned, only the tiger still lives in Java, and this form, one of the smallest of the living races, often is exceeded in size by the Pleistocene fossil tigers from the same island (Table 1).[9]

TABLE 1

Measurements of tiger upper carnassials (mm)

	Pleistocene Java	*Wadjak Java*	*Recent Java*
P^4 length	29.1-38.8	35.7	30.0-33.7
width	16.6-20.1	19.2	15.4-17.7

In connection with the problem of big-toothed prehistoric Man, it is of interest to note that the tiger is represented in the prehistoric Wadjak fauna of Java by a left P^4 (Leiden Museum, Dubois Collection, no. 3808b) that presents dimensions greater than those of the same tooth in the living Javan subspecies (Table 1). Wadjak Man too is characterized, among other features, by the relatively large size of his teeth.

7 D. A. Hooijer, *On Fossil and Prehistoric Remains of Tapirus from Java, Sumatra and China* (Zoologische Mededeelingen, Museum Leiden, vol. 27, pp. 253-299, 1947).

8 D. A. Hooijer, *On Fossil and Prehistoric Remains of Tapirus*, p. 269; Leiden Museum, Dubois Collection, no. 3808 a.

9 D. A. Hooijer, *Pleistocene Remains of Panthera tigris (Linnaeus) subspecies from Wanhsien, Szechwan, China, Compared with Fossil and Recent Tigers from other Localities* (American Museum Novitates, no. 1346, pp. 1-17, 1947).

The orang-utan from the "Upper Middle" Pleistocene of Patjitan in Java is stated by Von Koenigswald to be decidedly smaller than the "Lower" Pleistocene form from Sangiran in Java. This would be perfectly in accord with the rule. The Patjitan orang-utan is furthermore stated to be smaller than the prehistoric orang-utan from Sumatra; the latter was described by me in 1948 as having teeth that are 16 percent larger than those of the living Sumatran and Bornean subspecies of orang-utan. Finally, the Middle Pleistocene orang-utan of Patjitan is stated by Von Koenigswald to be smaller than the living form from Sumatra and Borneo too.

Unfortunately, Pleistocene orang-utan teeth from Java are still largely undescribed. The two orang-utan molars from the Middle Pleistocene of Trinil in Java first referred to *Pithecanthropus erectus* by Dubois have been dealt with by me;[10] these teeth did not seem to be different in size from those of the living Sumatran or Bornean orang-utan, suggesting that if the orang-utan had not become extinct in Java, it would have developed into a comparatively small-toothed separate subspecies just as we observe in other species in which the record is more complete. Further orang-utan material from the Pleistocene of Java has not been described yet, although a few specimens have been figured by Von Koenigswald.[11]

In Table 2 I give the dimensions of three fossil orang-utan teeth originating

TABLE 2

Measurements of orang-utan teeth (mm)

		Pleistocene Java	*Recent*		*Subfossil Sumatra*		*Pleistocene China*	
			M	E_m	*M*	E_m		
P^3	length	9.5	10.1	0.14	10.6	0.07	10.0	
	width	13.5	13.0	0.19	13.7	0.11	12.3	
M^2	length	14	12.2	0.18	13.8	0.06	15.5	
	width	15	13.9	0.18	15.3	0.07	15.2	
M^3	length	11.9	11.6	0.23	11.9	0.08	12.2	11.2
	width	13.7	13.2	0.19	13.7	0.10	15.5	12.5
M_3	length	15	13.6	0.27	14.5	0.10	17.2	
	width	12.5	12.3	0.20	12.9	0.09	14.0	

10 D. A. Hooijer, *Prehistoric Teeth of Man and of the Orang-utan from Central Sumatra, with notes on the Fossil Orang-utan from Java and Southern China* (Zoologische Mededeelingen, Museum Leiden, vol. 29, pp. 175-301, 1948).

11 G. H. R. von Koenigswald, *Neue Pithecanthropus-Funde 1936-1938: ein Beitrag zur Kenntnis der Praehominiden* (Wetenschappelijke Mededeelingen, Dienst van den Mijnbouw in Nederlandsch-Indië, no. 28, pl. II, fig. 16, pl. III, figs. 9-10, pl. XIII, figs. 9, 10, and 12, 1940).

from the Middle Pleistocene Trinil beds in Java, collected by Von Koenigswald and presented by him to the University of California Museum of Paleontology at Berkeley, in which institution I examined the specimens through the courtesy of Dr R. A. Stirton, to whom I tender my thanks in this place. The specimens are: P^3 dext., U.C.M.P. no. 33390; M^2 sin., U.C.M.P. no. 33389, and M_3 dext., U.C.M.P. no. 33388. The M^3 from the Pleistocene of Java in Table 2 is the worn left upper molar from Trinil formerly ascribed to *Pithecanthropus erectus* (Dubois) (Museum Leiden, Dubois Collection, no. 11621). The table contains further the means (M) and standard errors (E_m) of the corresponding measurements of the recent Sumatran and Bornean orang-utan, of the subspecies (*Pongo pygmaeus palaeosumatrensis*) from the prehistoric limestone caves of central Sumatra in the Dubois Collection at Leiden, and of the Middle Pleistocene subspecies (*Pongo pygmaeus weidenreichi*) from southern China. The last mentioned specimens are those described by Pei;[12] one tooth is known in each case except for M^3 of which there is one large and one small specimen. The data for the recent and for the subfossil Sumatran specimens as well as for the Pleistocene M^3 of Java are from my 1948 paper.[13]

It will be observed that the Middle Pleistocene teeth of the orang-utan from Java keep well in size with the recent ones, but that they are mostly above the average of the latter, and closer to the subfossil teeth from Sumatra. The latter, as said above, average 16 percent larger than the recent from the same island and Borneo, a difference in size that even stands the statistical test. Von Koenigswald states that the "Upper Middle" Pleistocene orang-utan from Patjitan in Java is smaller than the recent, and this fits nicely into the general picture; some specimens from Java (cf. the P^3 in Table 2) may be rather small indeed in comparison with the recent Sumatran and Bornean teeth.

As the matter now stands, the criticism given by Von Koenigswald has only supported the views that I expressed, and stimulated the present further clarification of my thesis. I can only repeat what I wrote before: Why should Man be an exception to the rule of progressive diminution in size in situ during the Quaternary? All the Malaysian mammals that I enumerated in my paper have not been replaced by, but have evolved into, the smaller-sized subspecies today inhabiting the same general areas.

We have seen that Wadjak Man of Java—his teeth being large on the whole as compared to those of living Javanese—was associated with a big-toothed tiger and with a tapir (the latter now extinct in Java) that have dimensions we should

12 W. C. Pei, *Fossil Mammals from the Kwangsi Caves* (Bulletin, Geological Society of China, vol. 14, pp. 413-425, 1935).

13 D. A. Hooijer, *Prehistoric Teeth of Man*, pp. 278 and 283.

expect to find for these animals, animals that have descended from individuals like those to which the comparatively large Middle Pleistocene teeth from the same island belonged. In these cases no migration is involved once the species reached the island some time back in the Pleistocene; there is evidence only of local extinction, and further microevolution in situ with diminution in size.

Migration in Man, of course, is possible on a far larger scale than in other terrestrial animals; his dispersal is less dependent upon various factors in his natural environment. This tends to complicate matters as far as Man (and the domestic animals) is concerned. Apparently much uncertainty still prevails concerning the size and number of migrations of Man over the Sunda area eastward, as recently discussed in this journal[14] and elsewhere.[15] However, in *Homo sapiens* as well as in other animals, not only in Malaysia but all over the world, on continents and on islands, prehistoric and Pleistocene remains are often larger than the corresponding recent material, and it is logical to suggest that the underlying causes, whatever they be, are largely the same.

In passing, it may be recalled that the big-toothed "Melanesoids" of Java and elsewhere in Malaysia (associated with Van Stein Callenfels' Bacson-Hoabinh culture complex) as a whole are dolichocranial, while the present peoples of Indonesia (Java) are more broadheaded. The real process seems to have been one of progressive brachycephalization in situ rather than one of replacement by another population some four thousand years ago as Von Koenigswald believes to have been the case. This evolutionary process is more advanced in some "racial groups" of mankind than in others; the whole problem has been discussed by Weidenreich in his important paper first published in this journal[16] to which I may refer.

Once more, then, however the large teeth of the present day Australians, Papuans, and Melanesians may be viewed phylogenetically, in my opinion they cannot be identified with the large teeth of Early Man of Malaysia because it is not too much to suppose that the teeth of the latter have become smaller in the course of time, paralleling the evolutionary drift of other mammals there.

Rijksmuseum van Natuurlijke Historie
Leiden, Netherlands

14 F. M. Keesing, *Some Notes on Early Migrations in the Southwest Pacific Area* (Southwestern Journal of Anthropology, vol. 6, pp. 101-119, 1950).

15 A. Riesenfeld, *Was there a Palaeolithic Period in Melanesia?* (Anthropos, vol. 47, pp. 405-446, 1952).

16 F. Weidenreich, *The Brachycephalization of Recent Mankind* (Southwestern Journal of Anthropology, vol. 1, pp. 1-54, 1945).

THE ARABIAN MATRIARCHATE: AN OLD CONTROVERSY

ROBERT F. SPENCER

INTRODUCTION

WHEN THE ARABS, under the banner of Islām, spilled out of their peninsular homeland to extend their conquests both eastward and westward from the Fertile Crescent and to found the 'Umayyad caliphate at Damascus, they already possessed a society in which loyalty to kin and tribe had been subordinated to the Muslim community and to the unyielding legalism of the Qur'ān.[1] In the person of Muhammad there is the crystallization of a pre-Islamic trend toward urbanization, which, with the increasing decentralization of various tribal estates, had become apparent in the Ḥijāz and other regions of peninsular Arabia.[2] The religion of Islām, far from being a mere "projection of the Bedouin mind,"[3] has been shown to be the culmination of far-reaching effects of commercial development, of shifting religious values, and of the changes in social and political structure in western Arabia.[4] An increase in trade promoted the founding of the city of Makkah (*ca.* 400 AD), and a climax in the pattern of social change was reached in the formation of the Muslim community at Madīnah (622-632 AD).[5] There can be little doubt that Arabia before Muhammad was an area culturally marginal to the centers of development of the eastern Mediterranean as well as to those of the southern littoral of the peninsula. In Muhammad and the Qur'ān, there is thus both the reflection of the effects of historical forces operative in Arabia itself and the distinctive rephrasing of diffused ideas which result in the particular configuration that is Islamic culture.[6]

For, of course, Islām is much more than the profession of faith that there is no god but God and that Muhammad is God's ambassador. Far more than any comparable movement, Islām succeeded in effecting a legalistic integration of economic, political, social, and religious institutions.[7] It has been suggested that the Muslim community successfully combines the patriarchal society of

1 Wolf, 1951, p. 352.
2 Von Grunebaum, 1946, p. 173.
3 Harrison, 1924, p. 42.
4 Wolf, 1951, pp. 329-330.
5 Lammens, 1926, p. 13.
6 Wolf, 1951, p. 352.
7 Gaudefroy-Demombynes, 1950, pp. 13-28.

the pastoral nomad with urban life.[8] The result is the distinctive cast of the culture of Islām.

It is evident that the Prophet Muhammad regarded his own role as that of a reformer rather than that of an innovator (cf. Qur'ān XLVI:9). There is general agreement that in fashioning the Muslim community he did not sharply diverge from the quasi-urban, quasi-nomadic patterns of society of the Arabia of his day.[9] In the Qur'anic regulation of community life, patriarchal institutions and the superior role of the male are emphasized. This may be regarded as the Islamic variation on the theme of father-right, coupled with monotheism, which is characteristic of the broad geographical area of the Near East.[10] The sharp contrast between the nomadic and settled peoples depends in large measure on the subordination by the latter of kinship to the religious community.[11]

Despite the father-right which characterizes the society of the Bedouin, the economic situation of desert life is such as to create a balance in sexual division of labor and to permit a high degree of freedom to women.[12] Urban living disrupts this situation, forcing women more and more into the background. Islamic law does permit inheritance by women, even though the portion they may receive may never be so great as that of male heirs.[13] The subordinate position of women undoubtedly becomes an integrative factor in the Muslim community, since the True Believer must accord protection to women, and *zakāh*, the prescribed almsgiving—one of the major premises of Islām—is directed in part to the benefit of the widow and orphan in the community.[14] Suggestions that women were somewhat more highly placed socially at the time of the rise of Islām appear fairly well borne out. Muhammad's evident dependence on his first wife, Khadijah, as well as the presence of numerous Muslim women of note (such as the Madinese poetess Jamīlah, d. 720 AD), whom one encounters in the first two centuries after the Ḥijra, might attest to this.[15] It is only with the 'Abbāsid period at Bagdad that the stricter seclusion of women begins,[16] and that the *ḥaram* system, together with the Byzantine institution of eunuchism, come into being in fuller force.[17] In Spain, too, following the establishment of the western caliphate,

8 Levy, 1933, vol. 1, p. 278.
9 Gaudefroy-Demombynes, 1950, p. 127.
10 Kroeber, 1948, p. 314.
11 Bräunlich, 1933, pp. 68-111.
12 Musil, 1928, p. 50.
13 Gaudefroy-Demombynes, 1950, p. 140.
14 *Idem*, pp. 104-105.
15 Hitti, 1937, p. 275.
16 *Idem*, p. 333.
17 Spencer, 1946, pp. 414-415.

Muslim women seem to have enjoyed greater freedom than their sisters in the east.[18] But, on the whole, the subordinate role of women is emphasized by the Qur'ān and it seems evident that Muhammad's social gospel reflects the patriarchal order of his day.

If it be concluded that the urbanized setting of the culture of Islām made for the social suppression of women, the nomadic situation admitting greater equality between the sexes, and if the founding of Islām be regarded as the outgrowth of a trend toward urbanization, it follows that in the pre-Islamic period the steps toward the lowering of the female status should be discernible. This point of view has been stoutly defended, not so much on the basis that the economic demands of settled life may create an imbalance in sexual role, but rather on the preconception that maternal institutions precede paternal ones. Islām even in the view of several modern writers, is held to be the culmination of the shift from matriliny to patrilineal society. It has also been maintained that among the Bedouin pastoral nomads, always lukewarm to Islām, are to be found survivals of the matrilineal stage, elements which, with a few exceptions, have been lost to the settled Muslim communities. Hitti remarks, for example, referring to the Bedouin tribal organization, that "the feminine names of certain clans show traces of the earlier matriarchal system."[19] Here is a survival of the classic sociological view that there is a priority of matrilineal institutions. According to this notion, there are coupled with matrilineal descent such features as totemism and exogamy, suggestions of group marriage and polyandry, and, in fact, all of the characteristics which, at the turn of the present century, were regarded as the inevitable corollaries of a maternal stage of ancient society.

At this late date, it is hardly profitable to return once more to the long discredited view that there are progressive stages of human society. Various data were being used fifty and more years ago to support the contention that the pre-Islamic Arabs had maternal institutions. But there is actually somewhat more to the society of nomadic Arabia than a cavalier denial of evolutionary premises admits. There are, on the one hand, both the unique social aspects of the Bedouin, past and present, and the fact that Islām, throughout its history, has allowed some rather distinctive forms of marriage; on the other, there is mention in the works of classical antiquity which lends support to the evolutionary reconstructions. Because contemporary anthropology has not entirely lost its preoccupation with history—although there are a number of signs to the contrary—it may not be amiss to explore the matter again, not only in order to redefine, in so far as

18 Hitti, 1937, p. 535.
19 *Idem*, p. 26.

possible, the nature of the social units of the pre-Islamic Arabs but also to scrutinize the indirect evidence which led to the propounding of an erstwhile Arabian matriarchate. An inquiry into the latter sheds some further light on early anthropological thinking.

SOCIAL ORGANIZATION IN PRE-ISLAMIC ARABIA

In the period which is known to the Arab historians as the Jāhiliyya—the Age of Ignorance, the unenlightened time before the Prophet—the Ḥijāz, Asir, and possibly, al-Yaman, in fact, the entire western littoral of Arabia as far north as Jerusalem and Damascus (however much these areas were affected by urbanizing trends) were inhabited by groups with a tribal organization of society.[20] Until the advent of Islām, the tribal estate continued to be characteristic of the bulk of the Arabian peninsula, remaining so among the pastoral nomadic Bedouin.[21] The tribe has generally been regarded as an *ursemitisch* complex.[22] Many investigators have equated tribe with sib (or clan) or with phratry (i.e. linked clans). The basis for this equation lies on the emphasis on unilineal descent with the concomitant feature of genuine or fabricated genealogy. And if this definition is accepted, exogamy should be an associated feature. But the fact of the marriage of parallel cousins, with its implications of endogamy, presents a sharp contrast to the usual concepts surrounding societies organized into sibs. Genealogical emphasis implies a lineage type of organization: it may suffice for the moment to accept the presence of patrilineal lineages and to reserve for fuller discussion below the question of a sib organization formerly in operation among the Arabs.

It seems clear that the pre-Muslim Arabs possessed a general lineage type of social organization. The basis for the presumed antiquity of this social form lies in a number of sources, such as in the relatively late Hebrew Pentateuch, in Arab histories and genealogical inscriptions, and in pre-Islamic poetry.[23] But from what is known of the Ḥijāz, or indeed, of much of the Near East as it was on the eve of the rise of Islām, it is clear that the cities and oases not only of Arabia, but also of Mesopotamia, Syria, and Palestine, were divided into quarters, in each of which an individual tribe was resident.[24] Such provision was made in Jerusalem, while in Makkah, the Quraysh—the unit (or "tribe") to

20 Ahrens, 1935, p. 150 *seq.*
21 Wellhausen, 1889, vol. 1, p. 4.
22 Goldziher, 1888, p. 40.
23 Sayce, 1897, p. 78; Blunt, 1903, p. xi *seq.*
24 O'Leary, 1927, p. 17.

which Muhammad belonged—held the center of the city.[25] Similarly, in Madīnah, two feuding tribes, the Aws and the Khazraj, divided the city between them and it was necessary for the outsider, the Christian or Jew residing there, to take sides and to become a partisan of either one group or the other.[26] Islām upset this situation and substituted religious unity for tribal loyalty. It is the opinion of some that tribal autonomy was already in a state of decay in the settled areas, since advantageous commercial relations could best be effected through political unity.[27] However this may be, it is evident that if the sedentary peoples of Arabia abandoned their tribal estates following the rise of Islām, the desert nomads were in no such way affected.

Hence any suggestion of a former mother-right must come from the peoples of the desert. Several geographical limitations are thus implied. The area of present concern is that of camel-nomadism, one in which a highly uniform pattern of culture appears.[28] This is found in the Arabian and Syrian deserts, and extends across the Red Sea into the Sahara. The similarity in social and economic institutions among the camel-raising peoples, the Bedouins, permits the delimitation of a specific culture zone, one which is essentially different in character from nomadic Central Asia and also from that of the semi-sedentary sheep-and-goat-nomads of the Middle and Near East.[29] The conservatism of camel-nomadism, the limitations which an unfavorable environment places on this type of life, as well as the resistance of these peoples to the urbanizing force of Islām, have created the prevailing opinion that even today pre-Islamic social structure is to be found among the Bedouins.[30] This contention is unquestionably a valid one; when the evidence on the pre-Islamic tribe is considered, it is possible to find a structural continuity.[31]

The geographical limitation applies further in that there is justification for regarding southern Arabia as a culture area separate from that of the Bedouin north. In Arabia Felix, which consists of the southern regions of the peninsula below the Empty Quarter, there is a strikingly different cultural configuration. Despite adherence to tribal fictions and the claim of Arab blood, al-Yaman, the Hadramawt, and related areas—with their ancient history of city states, suggestions of a class-structured society, and some racial and linguistic departures from

25 Lammens, 1924, p. 363.
26 Wellhausen, 1889, p. 4.
27 Margoliouth, 1916, p. 883.
28 Patai, 1951, p. 404.
29 *Idem*, p. 405.
30 O'Leary, 1927, pp. 19-20.
31 Oppenheim, 1939, vol. 1, p. 26.

the northern sections—may be regarded as wholly different from the Bedouin pattern.[32] The Muslim historians also viewed the southern peoples of the peninsula in a somewhat different light. The classic genealogies divide the southern from the northern Arabs, claiming for the latter descent from Ishma'ēl, while the former are regarded as the descendants of Qaḥṭān, son of the Biblical Eber.[33]

The nature of pastoral nomadism being such as to cause the periodic separation of local groups from a main assemblage, it may be expected that the ties of kinship should serve as an integrative force within the small nomadic grouping. Several such groupings together might make up a tribe, a more inclusive unit which, in the area of camel-nomadism, still depends on the strong sense of the blood tie enforced by genealogical reckoning.[34] Both the nomadic band and the tribe are recognizable as lineages on this basis. Reserving for the moment the question as to whether either or both may be justifiably viewed as sibs, it is possible to find in the genealogies a pivotal point on which to hang the discussion of the units of differing magnitude which appear in the social organization of the Bedouin.

Through the implied kinship of Ishma'ēl and Qaḥṭān, relationship through descent might be recognized for all the population of the Arabian peninsula.[35] While in theory this was so, time and circumstances, regardless of the fiction of ultimate kinship ties, caused the creation of local loyalties. These, characteristic of the band and the tribe, are presumably derived from the nomadic economic round. It follows that the tribe was conceptually a basic relationship group, tracing its descent from a common ancestor, whether real or fictional, and recognizing close ties of kinship on the basis of this descent.[36] The Pentateuch points to a Hebrew parallel of this social type. The twelve "tribes" of Isra'el were fictionally derived from the line of Abraham through Jacob, each lineage assuming its name from a founder who was ostensibly one of the sons of the patriarch, Jacob.[37] A similar pattern of lineage and descent, enforced by genealogy, applies to the Ḥijāz on the eve of the appearance of Islām. If the Quraysh may serve as an example, there is a group known to have been a component of a more inclusive federation of pastoral peoples, that of the Kimāna.[38] In turn, the Kimāna related to the Mudar, one of the two major groups of the 'Adnān,

32 Cf. Coon, 1943, pp. 193-194; Glaser, 1897, p. 7.
33 Nicholson, 1907, pp. xvii-xix, *passim;* cf. Gen. 10:24-25.
34 Montagne, 1932, pp. 65-76; Hommel, 1926, pp. 591-597.
35 Huart, 1903, p. 4; Abū'l-Fidā, 1831, p. 180.
36 Musil, 1908, pp. 26-28.
37 Sayce, 1897, pp. 79-81.
38 O'Leary, 1927, p. 19; (or Kināna, cf. Abū'l-Fidā, 1831, p. 180).

or Northern Arabs.[39] While there is some indication that these major enclaves may at one time have had some socio-political functio..s, it is clear that with the increasing urbanization of the Ḥijāz and the occupational specialization that this entailed, they served merely to enforce the concept of the remote blood-tie and to substantiate genealogy.[40] The Quraysh, as a sub-group, had settled in the Makkah area and had, by Muhammad's day, wholly abandoned its former nomadic way of life.[41] Mention is also often made of the various sub-groupings of the Quraysh, units frequently designated as clans. These appear to have been lineages within the Quraysh, groups whose solidarity was enforced by the claim of descent from a less remote ancestor. It is worth noting here that a sense of endogamy pervaded the Quraysh and might also pertain to its component lineages.[42] These groupings, such as the 'Ummayyah, the Makhzūm, the Nawfal, and other named lineages as well, were relegated to different quarters of the city of Makkah, and some economic differences, apparently relating to the increase in trade, as well as a situation of rivalry, had arisen between them.[43] By Muhammad's day, regardless of the shift by the Quraysh from nomadic to settled life, here were social institutions which clearly indicate the background of pastoral nomadism. This type of economy, and its associated form of social structure—to judge from the Qur'ān and from the works of the Arab lexicographers—finds parallels in the organization of society among the contemporary Bedouin.[44]

BEDOUIN SOCIETY

Nomadic communities today are divided into tents, each of which represents a family.[45] It is on this level that the first functioning unit of social structure appears. This is the *ahl*, the elementary grouping based on male kinship. Musil, in defining this unit for the Rwala Bedouin, remarks:

> The *ahl* (for which we shall use the word "kin" . . .) is a group fixed in relation to the individual only, a man's kin differing from the kins of his father and son. . . . A man's kin comprises his descendants to the third generation—that is, his sons, grandsons, and great-grandsons. It also includes his ascendants to the third generation. . . .[46]

39 al-Mas'ūdī, 1861-67, vol. 5, p. 221 *seq.*
40 O'Leary, 1927, p. 16.
41 Caetani, 1905, p. 148.
42 Stern, 1939, pp. 60-61.
43 Essad Bey, 1936, pp. 40-41.
44 Winckler, 1898, p. 48.
45 Bräunlich, 1933, p. 79; Musil, 1908, pp. 26-28.
46 Musil, 1928, p. 48.

The terms which are used to refer to the encampment of tents which forms a group of extended kindred are somewhat variable but the unit itself appears to be consistent. It is here that the concept "clan" tends to obtrude itself, identified by W. Robertson Smith as the *hayy,* and by others as the *qawm,* the *'ā'ila,* and sometimes as the *'ashīra.*[47] Regardless of term employed, here is a unit which has economic, social, and frequently, formalized political functions. This is the group which travels together in the nomadic round, is linked to certain grazing lands, and possesses common water rights.[48] Within the group, there may be certain *ahl* in which chieftainship is invested, the term *qawm* often being applied to the Bedouin group which has a chief *(shaykh).*[49] In the final analysis, this is the functioning relationship unit, an economic band, membership in which is reckoned by the closely drawn tie of blood.[50] Such kinship, in turn, depends on exact genealogical knowledge.[51] Several factors may operate to effect structural differences between bands. The local terrain may be such as to place limitations on the number of people who may effectively reside there: the band may thus consists of three tents or of three hundred or more.[52] The unit may be a single lineage, with or without a chief, or several closely linked lineages may move together.[53] The Arabic term, *'ā'ila,* refers to this structural situation.

From the band, it is possible to proceed to broader, more inclusive units. A number of kindred bands makes up a tribal convocation, *qabīlah.*[54] Within this unit, however, there may be several groups of more closely connected lineages, *'ashīra.*[55] But it becomes generally clear that a threefold arrangement of society exists. The first consists of the *ahl,* a personal lineage, at it were, relating to the realm of the immediate family. The second level is that of the lineage proper, a tightly knit grouping of extended patrilineal kin, the integrity of which is enforced by blood feud, common holdings, and a number of well-defined unifying symbols. The tribe, meeting at periodic intervals, may consist of several lineages but may also be coterminous with a single lineage.[56] Although there are Bedouin tribes which possess a paramount *shaykh,* the ultimate tribal grouping may be

47 Smith, 1903, p. 41 *seq.*; Bräunlich, 1933, p. 78; Jaussen, 1908, pp. 11-13 *passim.*
48 Smith, 1903, p. 41 *seq.*
49 Musil, 1928, p. 50.
50 Oppenheim, 1939, vol. 1, pp. 22-23.
51 Wellhausen, 1897, p. 228.
52 Bräunlich, 1933, p. 83.
53 *Idem,* p. 78; Jaussen, 1908, p. 111; Philby, 1928, p. 383 *seq.*
54 Montagne, 1932, p. 68; Bräunlich, 1933, p. 78.
55 *Idem,* p. 78.
56 Oppenheim, 1939, vol. 1, p. 22.

somewhat ephemeral.[57] Changes in population density, the interaction which inevitably occurs between the nomadic and settled groups, as well as such disunifying features as the blood feud, may serve to point up the importance of the local group as against the periodically occurring assemblage.[58] Regardless of the size of the enclave, it is to be emphasized that male kinship emerges as the dominant *leitmotif* in Bedouin society. Just as bands may relate to a major convocation, so also does conceptual kinship apply to inter-tribal relations on a larger scale. A pertinent example is supplied by Musil, who notes that the Rwala Bedouin belong to the group of tribes known as the 'Aneze, a major enclave which conceptually is founded in kinship.[59] Hence all groups which relate through male kinship to the 'Aneze are regarded by the Rwala as *banī al-'amm*, i.e. paternal cousins, however remote the point of common kinship may be.[60]

Apart from kinship as such, and the common economic activities of the nomadic round, there are other factors which effect local unity in a primary way. From the available evidence, the same factors appear to apply to the pre-Muslim nomads as operate among the contemporary Bedouin. The names of lineages, for example, are significant. Although a name attaches itself to the extended tribal convocation, the Bedouin professes himself to belong to a named lineage rather than to a named tribe.[61] Lineage names are somewhat variable and may stem from antiquity.[62] They are generally of the "nickname" type: he-panthers, lions, mice, the guardians, and the like.[63] Place names are also used, particularly in southern Arabia.[64] All such names are usually prefixed by *banū*, "sons-of."[65] Ibn Khaldūn says that these patronyms have become so stereotyped that exactness and the original meaning has been lost.[66] Musil notes that each of the Rwala lineages has an origin tale and this, together with genealogy, makes for the concept of membership within a tribal federation.[67] Related to the lineage name is the livestock brand or mark, *wasm*. This is a simple, usually geometric, device, the property of a given lineage.[68] By the *wasm* the group recognizes its own animals and is able to separate them from those of other lineages should the *qabīlah* convene. Brand

57 Montagne, 1932, pp. 65-76; cf. Seligman, 1918, p. 113.
58 Musil, 1908, p. 28.
59 Musil, 1928, p. 46.
60 *Idem*, p. 46.
61 Hartmann, 1909, p. 10.
62 Smith, 1903, p. 18.
63 Hartmann, 1909, pp. 350-393.
64 *Idem*, p. 10.
65 Oppenheim, 1939, vol. 1, p. 23.
66 Ibn Khaldūn, 1863, vol. 1, p. 273.
67 Musil, 1908, pp. 46-47.
68 Oppenheim, 1939, vol. 1, p. 22.

changing and livestock rustling are of course common practises, relating to the pattern of raiding, and may often lead to the start of the *ghazw*, the traditional blood feud, even between groups belonging to the same assemblage.[69]

There is probably no factor which so strengthens the bond of lineage membership as does that of the blood feud. While the *ahl* is vital to the protection of the individual, collective responsibility applies to the lineage as well.[70] Islām, in breaking down the lineage patterns, attempted to substitute a milder means of retribution for the inexorable dictum of life for life. But blood revenge continues as an integrating factor among the nomads of the desert, although King Ibn Sa'ūd has allegedly been more successful in curbing the vendetta than has Muslim law.[71] The point that lineages within a tribe may raid each other and engage in feuds offers additional support to the contention that tribal loyalties are ephemeral.[72]

A factor supporting the emphasis on blood relationship and the import of local loyalty is seen in marriage. Here the question of endogamy arises since marriages may take place within the lineage. The marriage of parallel cousins is theoretically required, the *bint 'amm*, father's brother's daughter, being the prescribed mate.[73] Technically, this may fall within the *ahl* but may be extended within the lineage. Musil notes that among the Rwala, the ideal marriage is that of a girl with her nearest possible male kin, those classed as *ibn 'amm*.[74] While this may be the father's father's brother's son's son, marriage between more closely related collaterals may also occur.[75] This pattern is so well engrained in Bedouin society that a women, in marrying anyone else, must first obtain the permission of her parallel cousins.[76] Suggestions of this type of marriage appear in the Qur'ān (XXXIII:49) and are borne out in pre-Muslim Arabia by what is known of the Quraysh genealogies.[77] In practice, every attempt is made to effect marriages between the children of men who stand to each other in an *'amm* relationship.[78] Oaths of blood brotherhood, clientage, and adoption are means by which the lineage circle may be increased and such marital arrangements made possible.[79]

69 *Idem*, p. 28.
70 Goldziher, 1888, p. 15 *seq.*; Smith, 1903, p. 27.
71 Philby, 1928, ch. 2.
72 Wellhausen, 1897, p. 228; cf. Thomas, 1932, p. 103.
73 Wellhausen, 1893, p. 436.
74 Musil, 1928, p. 137.
75 Levy, 1933, vol. 1, p. 147.
76 Stern, 1939, pp. 60-61; Musil, 1928, p. 137.
77 Stern, 1939, p. 62.
78. Levy, 1933, vol. 1, pp. 147-150.
79 Wellhausen, 1893, p. 436.

The reasons which the Arabs themselves give for marriages of this kind are numerous. The endogamous marriage is conceived to make for lineage solidarity.[80] Equality of birth is another factor which does not emerge in endogamy but which would have to be considered in exogamous unions. Through endogamy, the genealogy is left certain and ancestry ostensibly untainted.[81] Patrilocal residence would, moreover, lose to a group its daughters as well as the dowry involvements in marriage, or, at least, so the Arabs justify and rationalize their marriage practices.[82] Arab legends are replete with references to marriages of this type. It is said that the Quraysh explained their short stature and dark skin by the fact that they always carefully adhered to endogamy.[83]

While endogamous unions are apparently the most desirable, there is no hard and fast rule which enforces them. It is possible to marry into other groupings and this, in fact, is very often the case. Stern, in her review of the marriage statistics of Madīnah, finds that the largest proportion of marriages was contracted between members of the same lineage, but that the next largest group was composed of those marriages which took place between related lineages, presumably members of a more extended federation.[84] The same statements can evidently be made for the contemporary situation in Arabia.[85] The point about which greatest care is taken is that of purity of family and lineage background. Endogamy thus offers the best solution. This being the case, the pointed issue of endogamy versus exogamy really does not arise and is clearly subordinated to the necessity of preserving intact family reputation and pride in lineage.[86] But it is not to be implied that the Arab is careless in crossing certain lines: endogamy is desirable, but is limited, should it occur, to the parallel cousin, to the marriage of *bint 'amm* with *ibn 'amm*.

There are one or two other features in the culture of nomadic Arabia which deserve mention. A balance between lineage groupings—a further stabilizing force which operates to preserve genealogical fictions—is effected by the concepts of ritual kinship and treaty. While a weak lineage may be affiliated with a stronger for purposes of protection,[87] this situation has led to the development of the institution of clientage, a pattern of ritual adoption, which, according to

80 *Idem*, p. 437.
81 Wellhausen, 1893, p. 436.
82 Levy, 1933, vol. 1, p. 147.
83 Wellhausen, 1893, pp. 459-460.
84 Stern, 1939, pp. 60-61.
85 Charles, 1930, p. 83 *seq.*
86 Goldziher, 1888, p. 41.
87 Levy, 1933, vol. 1, p. 82 *seq.*; Musil, 1928, pp. 441-443.

Herodotus, at least, has its root in ancient Arabia (cf. Bk. III, ch. 8). A protected person, *dakhīl,* has not yet entered into any specific obligation to his protectors.[88] The client, *mawlā,* on the other hand, assumes an oath of blood brotherhood and may thus qualify for marriage within the group of his adoption.[89] While this is a relationship between stronger and weaker parties, an oath of blood brotherhood may likewise bind essentially equal individuals and groups. This is the *ḥilf* relationship, a kind of treaty, arranged for purposes of coöperation and mutual aid.[90] It is in the clientage relationship that some variations in marriage pattern may be expected to occur. The individual who achieves the adoptive relationship thereby qualifies for marriage with a woman who stands to him in an *'amm* relationship. The children of such a union would technically take their mother's lineage affiliation. It does not seem reasonable, however (a point reserved for fuller discussion below) to follow McLennan in terming this a *beena* marriage, after the analogous Singhalese case, where the contractor of a marriage renounces his own kin and assumes that of his wife.[91] Nor, under the circumstances, can it be regarded as a form of matrilocal residence. Even the often cited Old Testament parallel, where the patriarch Jacob not only performs bride-service but also remains afterwards in the territory of his mother's brother and father-in-law, Laban, fails to measure up to the accepted definitions of avunculate and matrilocal residence (cf. Gen. 29-31).

One other form of marriage might be included in a description of the marriage patterns of the Bedouin. Properly, the *mut'a* (usufruct or temporary) marriages do not apply to the nomadic peoples but they are significant in early Islām and are still seen in Makkah and Madīnah.[92] This is a temporary union, its length and obligations stipulated by contract, which may occur between pilgrims and women resident in the cities in question. The *mut'a* marriage, anathema to Sunnite orthodoxy, appears to be an especially sanctioned form of prostitution and may stem from the trend toward urbanization.[93] The form is mentioned here since to some it has suggested a survival of polyandry.

To summarize, certain features may be stressed which permit a somewhat more exact definition of the structure of society among the modern Bedouin. The tribal groupings of camel-nomadism suggest extended lineages, dependent, in turn, on the presence of a number of closely knit, patrilineal, and economically

88 Smith, 1903, p. 41.
89 Levy, 1933, vol. 1, p. 83.
90 Bräunlich, 1933, pp. 191-194; Wolf, 1951, p. 341.
91 McLennan, 1886, p. 101.
92 Snouck Hurgronje, 1931, pp. 124-125.
93 Levy, 1933, vol. 1, p. 131.

derived bands. Band equates with lineage on the local level, and the general concept of the extension of blood relationship is enforced by the emphasis on descent and genealogy. As additional reflection of the primary stress on ascendant kin, the endogamous marriage serves as a stabilizing force. Endogamy supports genealogy and reflects a departure from the conventional definition of sib organization. If it be concluded that the contemporary Bedouin lack a clan type of social organization, there is still the problem of determining whether, in the pre-Islamic situation, such units existed. Thus the modern Bedouin either offer a type of society which became modified as a result of the rise of Islamic institutions or else theirs is a form of social organization in which pre-Islamic institutions still persist. It is the former point of view which raises the question of an erstwhile matriarchate and the differing opinions concerning it.

THE PROBLEM OF THE MATRIARCHATE

The writers of classical antiquity, as is well known, were always attracted to what to them was the curious and bizarre. Herodotus, for example, was struck by the matrilineal institutions of the Lycians,[94] while Caesar's *Commentaries* devote some space to the question of polyandry among the Britons.[95] In the works of Tacitus, similar customs are ascribed to some of the remote Germanic peoples, while the Graeco-Roman geographer, Strabo (63 BC?-23AD?), discusses the Arabians in some detail. He does not mention the social life of the nomads at any length, but has the following to say regarding Arabia Felix ('Eudaimon):

> . . . Brothers are regarded more highly than children. . . . Common property is held by all kin, although the eldest is master of it. One woman, moreover, is wife to all, the first man going in to have intercourse with her placing his staff by the door, since custom demands that each man carry a staff. Nights, however, she gives to the eldest. Hence, all are brothers of all. They also have intercourse with their mothers . . .[96] [translation this writer's].

While the last sentence—regarding which no further explanation is given—can probably be dismissed as a bit of ancient sensationalism, the description given is remarkably reminiscent of polyandry such as it has been described for the Toda, and which reflects McLennan's fraternal polyandry of the Tibetan type.[97]

It is quite evident that Strabo refers to southern Arabia where, it must be admitted, the genealogical analyses—derived from examination of inscriptions

94 How and Wells, 1912, vol. 1, p. 134; Bachofen, 1926, pp. 63-115.
95 McLennan, 1886, p. 106 (cf. *De Bello Gallico*, V:p. xiv).
96 Strabo, 1930, Bk. VII:pp. 364-365.
97 McLennan, 1886, pp. 99-100.

by Glaser, Nielsen, and others—appear to affirm an ancient polyandry.[98] As Glaser points out, however, the children of two brothers under a levirate may be listed as the children of both with the result that one might be led to the erroneous conclusion that polyandry once existed.[99] No such evidence, moreover, is available for northern or central Arabia, except for such distinctive features of marriage as may provide a basis for reconstruction. It was Bachofen, with his encyclopedic *Mutterrecht,* which leaned so heavily on the Classics, who set the tone for the investigators of the early institutions of the Semites.[100] And in England, too, McLennan, the Scotch barrister (apparently quite independently of Bachofen[101]) had evolved his scheme of the parallel development of human institutions in which exogamy, female kinship, both associated with totemism, are viewed as fundamental early traits.[102] It remained for W. Robertson Smith to apply McLennan's concepts to the pre-Islamic Arabs. This he did in his memorable *Kinship and Marriage in Early Arabia,* a work first published in 1885.[103] Nor was he alone in his investigations of the matter. Edward B. Tylor, although not concerned as an investigator, but influenced by the work of the Dutch scholar, G. A. Wilken, had commented favorably on survivals of maternal institutions among the Arabs, when, in 1884, he gave the Presidential Address before the Anthropological Section of the British Association for the Advancement of Science at Montreal.[104] Similarly, on the continent, Arabic scholars, presumably under Bachofen's influence, began to interest themselves in ancient Arabian polyandry and the features ostensibly associated with it.[105] It may be of interest to offer a brief summary of the various points of view regarding the matter.

1. *Matriliny.* Smith based his observations largely on the tribal genealogies collected by Muslim historians. He questions the claim of the Arab genealogists that tribal institutions had always been patriarchal.[106] Pointing out that the words for "mother" in Arabic and Hebrew respectively are *'umm* and *'em,* Smith notes the general extension of the meaning of the root to denote such concepts as "stock," "race," and "community."[107] The Arabic collective *'ummah* has the meaning "community," signifying, under Islamic usage, a neighborhood with a common *'imām,* or perhaps more specifically, an area served by a mosque. Smith

98 Glaser, 1897, p. 7; Nielsen, 1927, p. 56; Hommel, 1926, p. 697.
99 Glaser, 1897, pp. 7-8.
100 Bachofen, 1926.
101 Lowie, 1937, pp. 43-49.
102 McLennan, 1886, p. 50 *seq.*
103 Smith, 1903; cf. Smith, 1880.
104 Tylor, 1885; Redhouse, 1885, p. 275.
105 Cf. Nöldeke, 1886; Wilken, 1884, etc.
106 Smith, 1903, pp. 3-4.
107 Smith, 1880, p. 78.

is also struck by the fact that in many parts of Arabia, the term *baṭn*, the belly, or more exactly, the mother's belly, may be a synonym for *ḥayy* and *qawm*.[108] Allied with this, moreover, is the concept of kinship which may be expressed in Arabic by the term *raḥim*, the womb.[109] Proceeding from such semantic considerations, Smith concludes that the *ḥayy* was a matrilineal, named, totemic, exogamous, kinship group, which, by 600 AD, had begun to take on the patrilineal aspect, a feature enforced by Muslim law.[110] The proof for this, according to Smith, lies in the numerous survivals evident in both the Qur'ān and in contemporary Bedouin life.[111] Metronymic clans, the relationship with the mother's brother (a point noted below), and cases of adoption by the mother's group in the event of exogamous marriages are cited by Smith, among numerous other features, to support his claim of the erstwhile existence of female kinship.[112] Moreover, the names of the units—viewed by Smith as maternal clans—were connected with plants, animals, natural phenomena, and the like, and were thus suggestive of survivals of totemism.[113]

2. *Totemism*. The religion of the Jāhilīyya age was quite variable. In it are seen fragments of Christian, Gnostic, and Jewish beliefs as well as concepts of demonolatry, and reverence directed toward heavenly bodies and natural prenomena, not the least of which was the Black Stone of Makkah itself.[114] Since the Bedouin of the pre-Muslim age were notoriously irreligious—as indeed, they are today—Smith took the view that the totem was the central religious feature and that clan names were totem names.[115] The fact that such names today are frequently feminine in grammatical gender was sufficient indication to Smith of totemic maternal institutions.[116]

3. *Polyandry*. G. A. Wilken, the Dutch scholar, writing shortly before Smith, but influenced by Smith's earlier paper, followed essentially the same line of reasoning. Totemism and female descent were viewed by him as arising from an earlier promiscuous stage.[117] Polyandry, both to Smith and Wilken, was a logical aspect of the matrilineal situation. The *mut'a* marriage was regarded as a polyandrous survival, and both authors treat the widely diffused levirate of the

108 Smith, 1903, p. 37.
109 *Idem*, p. 32.
110 *Idem*, pp. 5-7.
111 Cf. Winckler, 1898, p. 50.
112 Smith, 1903, pp. 156-158.
113 Smith, 1880, p. 79; Wilken, 1884, p. 4.
114 Margoliouth, 1914, p. 99.
115 Smith, 1903, p. 221.
116 *Idem*, p. 31.
117 Wilken, 1884, p. 7.

Semites as indicative of a former fraternal polyandry.[118] This institution was considered to stem from female infanticide, a practise which both Wilken and Smith were able to support from the Qur'ān (cf. Sūrah LXXXI, 8: "And when the child who was buried alive shall be asked for what sin she was slain . . .") and from the historians.[119] So frequent, in fact, are the Qur'anic references to female infanticide, especially by burial alive, that Levy reaches the conclusion that here was a pre-Islamic religious rite, involving human sacrifice.[120] Both Smith and Wilken utilize Muhammad's prohibition of infanticide, his reluctant admission of *mut'a* marriages, and his injunction of the levirate as evidence for the shift in institutional patterns.[121]

4. *Avunculate.* A suggestion to the effect that a special relationship might apply to the *khāl*, the men designated as mother's brothers, is regarded by Smith and Wilken as proof of a vanished avunculate.[122] The argument was supported by the blessings or curses which today might be directed toward another's maternal uncle. Additional mention of the maternal uncle in proverbs is viewed by the writers as sufficient evidence of the erstwhile presence of avuncular authority.[123] Tylor's Section H address emphasized Wilken's point: "The Arabs of today hold that the nearest relative is not the father but the mother's brother. . . ."[124]

The foregoing points will be recognized by contemporary anthropologists as predicated on the classic evolutionary view of progressive stages of society. Both Smith and Wilken recognize a problem in respect to the point at which occurs the shift from maternal to strongly paternal institutions. Smith has a ready explanation. The intermediate state—half maternal, half paternal—is discussed by him in the development of the so-called *ba'al* marriage, essentially fraternal polyandry with recognition of male rather than female kinship.[125] From this there is an easy transition to the patriarchate and male kinship and to the gradual subjection of women to men. The result is the polygyny of established and sanctioned Muslim practise. For economic reasons, endogamy began to replace exogamy.[126] The various marriage forms, totemic names, avuncular

118 *Idem*, pp. 44-45.
119 Smith, 1903, p. 295.
120 Levy, 1933, vol. 1, p. 131.
121 Wilken, 1884, pp. 44-45; cf. Stern, 1939, p. 70.
122 Smith, 1903, p. 58.
123 *Idem*, p. 59.
124 Tylor, 1885, p. 905.
125 Smith, 1903, p. 92 *seq.*
126 *Idem*, p. xi.

references, all of which survive today, are proof positive of the earlier stages.[127] Wilken, on the other hand, is somewhat extreme. He expressly states that just before the rise of Islām the entire situation is suddenly reversed in favor of the patriarchate and the bride price, both of which reflect the now subservient position of women.[128]

It is of interest to note that the hypotheses advanced by Smith and Wilken, however much they reflected the social historical philosophy of the day, were by no means generally accepted. The challenge came, in fact, not from the anthropologists of the time, but rather from a fairly wide circle of Semitic scholars both in England and on the continent. The criticism has been levelled against the Muslim historians that they were too forgetful of the pre-Islamic situation and, except for some interest in the accepted accounts of the Biblical patriarchs, were principally concerned with genealogy.[129] It was the consensus of such writers as Goldziher, Nöldeke, Redhouse, Hartmann, and others that only two points emerge from the rather obscure pre-Islamic past. The first lies in the genealogies which show the various tribes as descended through the male line from persons, actual or fictitious, who constitute a kind of original *paterfamilias*.[130] The second appears in the suggestions of group solidarity which is borne out by the stirring accounts of blood revenge and the persistence, from earliest times, of the collective responsibility incumbent on each member of the group.[131] All other evidence is thus essentially indirect. It has been demonstrated that the various units of contemporary Bedouin society are in some measure indicative of the past. Tribal organization of the type described, with such features as parallel cousin marriage, seems pretty well documented for Makkah and Ta'if at the time of the rise of Islām.[132] The Biblical references, often quoted in support of an earlier matrilineal situation, are too cryptic and lack sufficient detail to permit definition. Hence the arguments of Wilken and Smith are dependent on reconstruction derived from what they regarded as survivals still apparent in post-Islamic society. Considering the major points used by them to defend their respective hypotheses, one may note the objections advanced by their contemporaries.

The problem of totemism has been so long settled that it is scarcely necessary to do more than allude to it. The penchant of the Arabs for nicknames is noted

127 *Idem*, p. 260 *seq.*; Wilken, 1884, pp. 42-43.
128 *Idem*, p. 62.
129 Della Vida, 1944, p. 27.
130 Nicholson, 1907, pp. xviii-xx; Nöldeke, 1886, p. 149.
131 Goldziher, 1888, p. 50 *seq.*
132 Lammens, 1922, p. 180.

by Nöldeke, who points out that in such an instance as a tribe calling itself *Banū 'Asad,* "Sons of a Lion," a host of varied circumstances might bring about the appellation.[133] He also points out that in Arabic, a collective noun may be rendered in the feminine.[134] Hence, to speak of *Banū 'Asad* as *'Asadah,* thus referring to the "Lion" tribe, is good Arabic usage.[135] Smith ignored linguistic function in this instance and it appears that no further evidence can be adduced to prove this a female eponym.[136]

The arguments advanced by the Turkish scholar, J. W. Redhouse, writing before the publication of Smith's work, takes exception to Wilken's conception of the subdivisions of an Arab tribe. Pointing out that the Arabs arranged the various units according to metaphorical comparison with the human body and its parts, he follows the lexicographers in listing the component sections. A *hayy* (a derivative of the root "to live") this thus the great tribe, the "living individual."[137] A smaller division of this is the *qabīlah,* a reference to the cranial bones; and a still smaller unit, the *baṭn,* "belly," may occur within this.[138] The analogy can be followed through units of increasingly lesser magnitude. Redhouse thus finds the subdivisions couched in the metaphor of Arabic and in no way proof of a former matriliny.

And it is Redhouse who takes exception to the avunculate which Wilken, and later Smith, believe to have existed. His argument here is somewhat less convincing but his attempts to explain away the common curse against the maternal uncle are worth quoting:

> The moral qualities inherited by a man from his father are commonly well known to the public. Should any of the points of a man's character differ from those known to belong to his father, good or bad, they will most probably be seen foreshadowed in the character of the mother's brother, though not open to public observation in the mother herself. The illogical mob at once attributes to some occult influence of the uncle what is naturally inherited from the unobservable mother, hence their blessings or curses on him.[139]

Redhouse further defends his statement by citing the proverb: *'irqu 'l-khāli lā yanāmu,* "The character of the maternal uncle does not sleep."[140] The inference

133 Nöldeke, 1886, p. 160.
134 *Idem,* p. 169.
135 *Idem,* p. 170.
136 Hartmann, 1909, p. 198.
137 Redhouse, 1885, pp. 281-282.
138 *Idem,* p. 282.
139 *Idem,* p. 286.
140 *Idem,* p. 287.

is that the father of a man is too sacred to debase; his mother or her family are of less consequence. It is odd that Redhouse makes no point of the fact that the maternal uncle, by the rule of parallel cousin marriage, will stand in a cousin relationship to the father. The common curse, which thus avoids the acme of Arabic epithet, the "God damn your father," still succeeds in involving paternal antecedents.

In polyandry, both Smith and Wilken find grist for their mill. Strabo's reference, the Qur'anic mention of female infanticide, the levirate, and the *mut'a* marriage support the argument. Hartmann, who rejects the evolutionary reconstructions, finds no direct mention other than that of Strabo, and is dubious about the evidence of the Sabaean and Himyaritic inscriptions.[141] Redhouse, although he admits that polyandry might have been practised, questions the connection between polyandry and matrilineal descent.[142] And in essence this view is supported by Nöldeke and Goldziher. Redhouse's summary is interesting:

> One may agree with advanced anthropologists on *á priori* hypotheses, in surmising that, wherever their primitive abode may have been, among Semitic peoples, as among others, at some immensely remote period . . . a matriarchate system may have prevailed. . . . But Professor Wilken's labored arguments fall very far short, in the treatise under consideration, of the startling and utterly groundless assertion of the very learned and indefatigable President of the Anthropological Section of the British Association at Montreal, to the effect that among the Arabs to this day survives the most matriarchal idea that one's nearest relative is not one's father but one's maternal uncle.[143]

Tylor wrote to Redhouse on the matter and their correspondence remains for the archivist. Wilken, in turn, wrote a strong statement of rebuttal directed at Redhouse and his other critics but added little new.[144] There the controversy appears to have ended, but, as has been shown, Smith's point of view continues to influence a number of researchers in the Semitic and Oriental fields.

CONCLUSION

As stated earlier in the present paper, it has not been the intention here to refute again outmoded evolutionary hypotheses. The question remains as to whether, in the light of contemporary findings, it is possible to clarify the situation somewhat more exactly. Redhouse and the others who took exception to the

141 Hartmann, 1909, p. 199.
142 Redhouse, 1885, pp. 277-278.
143 *Idem*, pp. 290-291.
144 Wilken, 1885.

work of Wilken and Smith emerge as figures of interest, considering the day in which they wrote. Although giving lip-service to the theory of unilinear evolutionism, the critics are far from convinced. As Redhouse remarks:

> Whether Bachofen has correctly stated the case of the Nairs, or not, as appears very possible, matters very little; the Nairs are not the Arabians, and Professor Wilken's matriarchate is but a gratuitous, arbitrary hypothesis.[145]

It follows from the various dissections of the evolutionary evidence made by the contemporaries of Wilken and Smith that their arguments can now be viewed with other anthropological antiques. It is rather the work of such men as Redhouse, who, although trained in disciplines other than the sociology of the day, presage an approach based on social structure and function.

And, as one places the fragments of evidence of pre-Islamic society against the framework of the social organization of the modern Bedouin, and views the contemporary situation as a structural whole, maternal survivals seem utterly out of place. Each one, ostensibly stemming from pre-documentary antiquity, permits some alternative explanation. As the documentary evidence is considered, it is seen that the poems and genealogies of the Jāhilīyya age stress the paternal tie, and that philology can explain away the bulk of what the evolutionists regard as survivals of the remotest past. Paternal kinship, the *bint 'amm* marriage, the blood feud, in fact, tribal organization as seen today, all are mentioned in the Qur'ān. Such proverbs as "Whoso shames his *bint 'amm* will have of her no sons" point to a well integrated institution of parallel cousin marriage.[146] Muhammad, in his role as political unifier, attempted to make certain changes. If nothing else, he seemingly caused the tribes of his day to hold fast to their social organization and to resist the changes in it which the urbanizing force of Islām might bring.

It is apparent that both now and in the past, units of varying size, however designated, rise to confront the investigator. The problem of their differentiation becomes acutely difficult if it be decided that at a specific level sibs or clans are encountered. The conventional definition of sibs applies neither to the contemporary nor the pre-Islamic situation. It is true that unilineal organization is a most prominent feature in the social structure of the Bedouin, but the assumption that this is the paramount characteristic has been wholly misleading in that it eclipses a still more important consideration, that of the emphasis on genealogy and descent. Thus while the nomadic pattern has been universally identified with

145 Redhouse, 1885, p. 281.
146 Snouck Hurgronje, 1925, vol. 5, p. 18.

the concept of tribe, it is not possible, except in terms of genealogical reckoning, to offer an exact definition of the institution. In the sections above, the terms lineage and band have been employed and with apparent justification. It is evident that the pastoral nomadic life of the Bedouin hinges in its basic structure on the socio-economic band. The definition which Steward offers for the patrilineal band clearly applies to the society of the contemporary Bedouin and, as may be implied, to the tribal estate of pre-Muslim Arabia as well:

Patrilineal band: politically autonomous, communally land-owning, exogamous, patrilocal, patrilineal in land inheritance, and consisting theoretically or actually of a single lineage, which, however, comprises several households or elemental bilateral families.[147]

Here, a discernible exception is the feature of endogamy. If, however, the apparent concern is for land, for purity of stock, untainted genealogy, and for such aspects as the levirate, the resulting institution is readily understood. It is the aristocracy of blood which is still a potent force in the world of Islām.

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147 Steward, 1936, p. 331.

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